THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

VOL. XLII. SATURDAY, JANUARY 20, 1883. No. 3.

ORIGINAL LECTURES.

PHILADELPHIA COUNTY MEDICAL SOCIETY LECTURES.

ON THE PHYSICAL EXPLORATION OF THE LUNGS BY MEANS OF AUSCULTATION AND PERCUSSION.

A course of three lectures delivered by invitation before the Philadelphia County Medical Society.

BY AUSTIN FLINT, M.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE AND OF CLINICAL MEDICINE IN THE BELLEVUE HOSPITAL MEDICAL COLLEGE, NEW YORK.

LECTURE III.

AUSCULTATION, CONTINUED AND CONCLUDED.

Delivered January 13, 1883.

THE adventitious sounds or râles produced by acts of respiration, and the vocal signs, will furnish the topics to be considered in this lecture.

MOIST AND DRY BRONCHIAL RALES.

The bronchial râles, moist and dry, were studied and explained by Laennec so completely and accurately that they need at the present time, in these regards, no modifications nor additions. Diversified as are the sounds, he reduced them to three signs, namely, the mucous (more correctly called the moist bronchial or bubbling râles), the sonorous, and the sibilant râles. These are distinct from the crepitant, the subcrepitant, and the crackling râles, which require separate notice. The crepitant and the crackling râles are distinguished as not bronchial but intra-vesicular in their source. It is interesting to contrast this simple division by Laennec with the over-refinement by Fournet. That author describes five varieties of intra-vesicular râles, five varieties of extra-vesicular râles, and four varieties of bronchial râles. Skoda aimed at simplicity in his division, but it is in striking contrast to the simple arrangement by Laennec. Skoda divided the râles which have been referred to, into: 1st. A vesicular râle; 2d. A consonating râle; 3d. A dry crepitating râle; and, 4th. Indeterminate râles. The effect of this division and nomenclature is to substitute confusion and error for clearness and correctness. It is needless to mention here the physical conditions which the moist and dry bronchial râles represent. I will make one practical remark respecting the clinical significance of the moist or bubbling (or, as called by Laennec, the mucous) râles—namely, they are often wanting in bronchitis affecting the larger and medum-sized tubes, for the reason that adhesive mucus and solid sputa are not suited for bubbling. These râles occur when the bronchial tubes contain pus, serum, blood, or liquefied tuberculous deposit.

It is pleasant to find in Skoda's treatise a statement, with respect to these râles, of an important practical fact. I quote the statement referred to as follows: "The pitch of a râle generally corresponds to that of the respiratory murmur which is either replaced by or accompanies it." Again, "The acute, large, or unequal bubbling thoracic râles indicate the same condition of the lung tissue as bronchophony and bronchial respiration." The importance of this fact is perhaps not always appreciated by auscultators. It enables the auscultator to determine by the râles alone, if respiratory sounds be, as they sometimes are, absent, whether solidification of lung exists or not. For examples, the bubbling râles in capillary bronchitis and pulmonary œdema are always low in pitch, denoting non-solidification of lung. These râles are high in pitch in a lobe solidified by pneumonia or by a phthisical affection. It does not seem to me easy to explain this effect upon the pitch of râles by solidification of lung, but it is not less true, on that account, as a clinical fact determined by observation.

The moist bronchial râles are easily illustrated artificially as follows: Attach to a Davidson's syringe a long India-rubber tube, or a series of tubes differing in size. After passing a little water through the tube or tubes, bubbling sounds are produced by a current of air for several hours. To observe these the tube should be held close to the ear, and other sounds excluded by covering the ear and the tube with the palm of the hand. This experiment shows that a very little liquid is sufficient to produce much bubbling, and therefore the abundance of moist bronchial râles is not evidence of much liquid.

The dry râles, sibilant and sonorous, can be illustrated with India-rubber tubes, either attached to Davidson's syringe or using the breath, by contracting the tubes at certain points. This may be done by either applying ligatures or compression with a forceps. It requires, however, some pains to secure the precise amount of contraction of the tubes necessary for the production of whistling and snoring sounds. The experiment, thus, while it demonstrates the mechanism of these râles, is defective in some accessory physical conditions which are involved in their production in cases of asthma and bronchitis.

SUBCREPITANT AND CREPITANT RALES.

Laennec described a moist crepitant râle, a subcrepitant râle, and a dry crepitant or crackling râle. His descriptions, as far as they go, are exact, but he failed to draw a sharp line of distinction between the râles now well known as the crepitant and the subcrepitant. The error of attributing the crepitant as well as the subcrepitant râle to the bubbling of liquid, probably occasioned perplexity. The fact that the crepitant râle is heard with inspiration and never with expiration, would not occur to him as probable if both were bubbling sounds; hence he failed to recognize this important distinctive point. That he confounded the crepitant and the subcrepitant râle, seems certain; yet he regarded the crepitant râle as the pathognomonic sign of pneumonia. In this respect he was in advance of his commentator, Skoda, who declared that the crepitant râle is only occasionally limited to inspiration, and that "no distinctive line can be drawn between crepitating, subcrepitating, and mucous râles." This must seem a remarkable statement to the auscultators of to-day; at all events, to those of this country. Not less remarkable is Skoda's statement that he has not often observed, in cases of pneumonia the crepitant râle as described by Laennec.

The true explanation of the mechanism of the crepitant râle was first given by an American writer, the late Dr. E. A. Carr, of Canandaigua, New York, in a communication published in the *American Journal of the Medical Sciences*, in 1842. This explanation attributes it to the separation of the walls of air vesicles in

the act of inspiration, more or less of the walls having become *adherent* at the end of the act of expiration, from the presence of adherent mucus. It is probably more accurate to say that the sides of the ultimate bronchial tubes or bronchioles adhere at the end of inspiration, and are separated by the act of inspiration; hence, it is not strictly correct to call the crepitant râle a vesicular râle. It is not a bubbling sound, and the application to it of the term moist, is one of the few instances in which Laennec's observations were influenced by his reasoning. A true crepitant râle is characterized by dryness; but it is to be considered that Laennec did not discriminate sharply the crepitant from the subcrepitant, the latter being a moist or bubbling sound. After the lapse of forty years the explanation by Carr has made considerable headway as regards its general adoption. The explanation is so simple, it accounts for the peculiar characters of the sign so completely, and its correctness may be rendered so demonstrative by artificial illustrations, that it would seem as if it should at once have been adopted. Carr's imitation of the râle was by moistening the thumb and forefinger with a little mucilage, and alternately pressing them together and separating them, held close to the ear. As already stated, a crepitant râle may be obtained by auscultating directly a healthy lung after its removal from the body. The illustration is better after twenty-four hours than shortly after the lungs are removed. But the most perfect illustration was by means of an article made ten or twelve years ago, of India-rubber, to serve as a substitute for a sponge. This article was in structure like a very fine sponge; when compressed and then allowed to expand, holding it close to the ear, the representation of the crepitant râle was as perfect as possible. This illustration furnished demonstrative proof that the râle is not a bubbling sound, and that it is produced by the separation of adherent surfaces, for in using the article to illustrate the sign, not the least moisture was required.[1]

The subcrepitant râle may be illustrated with the lungs of the calf or the sheep. After having obtained, by application to the lungs of the binaural stethoscope, the crepitant râle, which, as has been seen, may be in this way illustrated, a certain quantity of liquid (water or glycerine) is to be poured into the trachea. Now, imitating respiration by inflating with the bellows, the subcrepitant râle is beautifully represented. The bubbling is extremely fine, and is associated with crepitation. It is heard with the current of air which represents expiration as well as that representing inspiration. A similar association of the crepitant with the subcrepitant râle is by no means uncommon in pneumonia, and is, perhaps, the rule in the resolving stage of that disease. It is probable that this fact contributed to Laennec's confusion as regards these two râles.

A recent theory attributes the crepitant, the subcrepitant and, indeed, other bubbling râles, to intra-pleural exudation and adhesions. The experiment just stated demonstrates the fact that each of these râles may be produced within the lungs removed from the body. Autopsical examinations in cases in which these râles have been observed during life, show the existence of physical conditions analogous to those in that experiment. The râles are found in cases in which examinations after death show no pleural exudation or adhesions. On these data may be based the assertion that the intra-pleural theory of the production of these râles is, to say the least, purely gratuitous. This is not saying that the stretching of fibrin, or of newly formed tissue between the pleural surfaces, may not sometimes produce sounds which simulate these râles. I cannot, however, think that the ear of an experienced auscultator can be thereby often deceived.

The pleural friction sounds with which every practical auscultator is familiar, have characters which are sufficiently distinctive. They are expressed by such terms as grazing, rubbing, creaking, grating, rasping, and not by bubbling or crackling. They are irregular in their connection with the respiratory acts, occurring with some and not with other successive acts; now heard in inspiration and now in expiration; sometimes at the beginning and at other times near the end of either inspiration or expiration; in some instances being continuous, in other instances interrupted or jerking, and not infrequently disappearing temporarily after repeated forced respirations. These are not characters in common with the crepitant or the subcrepitant râle. A highly distinctive feature is their apparent superficial seat. They seem to be produced directly beneath the ear applied to the chest, or the stethoscope. Râles produced within the air tubes or vesicles differ in this regard. Of the clinical significance and importance of pleural friction sounds, it is unnecessary to speak.

Laennec described a râle under the name "dry crepitant, with large bubbles or crackling" (*râle crepitant sec à grosses bulles ou craquement*). He stated, as a distinctive feature, that it is limited to the inspiratory act. He supposed it to be a diagnostic sign of pulmonary emphysema. There is an obvious incongruity in calling the sign a dry râle, with large bubbles. The term crackling is more definite. At the present day this râle is not generally recognized as a distinct sign. Skoda admitted its existence, but remarked, somewhat flippantly, that they who had failed to recognize it had not lost much. A râle such as Laennec described is the crackling at the summit of the chest, which is one of the accessory signs in the early stage of phthisis. It is sometimes heard at the summit of the chest on both sides in healthy persons at the end of a very deep inspiration. It is certainly not of much diagnostic value in pulmonary emphysema, but I have sometimes met with it occurring in connection with that affection. It has seemed to me to originate in the portions of lung not having become emphysematous. I have regarded it as an abnormal exaggeration of the vesicular quality of the inspiration in the normal respiratory murmur.

Metallic tinkling, a sign with characters so distinctive that it is recognized at once from a description of them, has given rise to diversity of opinion and much discussion in regard to its mechanism, there being no lack of agreement in respect of its clinical significance. Jacob Bigelow, in 1839, demonstrated by a few well-devised experiments that the dropping of liquid in a space containing liquid and air gives rise to the sign. This was Laennec's explanation. Bigelow, however, demonstrated that the sign was also produced by the explosion of bubbles on the surface of a liquid, within a cavity containing both liquid and air. This was the explanation given by Bean, Dance, and Spittal. These two explanations require the presence of liquid as well as air within either the pleural cavity or a pulmonary excavation. A third mode of production is the bursting of bubbles of air at the mouth of a fistulous opening into the pleural cavity, or at the point of communication of a bronchial tube with a pulmonary excavation. It is easy to demonstrate the correctness of each of these explanations. The sign may be produced by either of the three modes, and, clinically, each is applicable to certain instances. For this demonstration we may employ the India-rubber bag be-

[1] A similar article is now made and used by artists. Something, however, is combined with the India-rubber, making it better adapted for its use as a sponge, but impairing very much its fitness to illustrate the mechanism of the crepitant râle.

longing to the modern foot-ball, which has been found serviceable in illustrating tympanitic resonance on percussion, and amphoric respiration. If the opening into this bag be connected with an India-rubber tube of sufficient length, the bag partially filled with a liquid and inflated, metallic tinkling may be produced by holding the bag so that the opening is dependent, and causing bubbles at the surface of the liquid by introducing air through the tube from the mouth. It will be found that the quantity of liquid within the bag must not be large, otherwise gurgling is produced instead of the tinkling sounds. Tinkling sounds may be produced by making the other end of the bag dependent, and thus causing drops to fall from above to the surface of the liquid. Shaking the bag will also produce the sounds. Now, if the liquid within the bag be allowed to escape, and a few drops remain in the tube, on blowing into the latter and holding the bag close to the ear, excellent examples of this sign are obtained. This last experiment gives the best illustration, and it is probable that it exemplifies the most frequent of the modes in which the sign is produced clinically.

We are now to consider vocal signs, and, first, the signs which are incident to the laryngeal or loud voice.

The names bronchophony, pectoriloquy, and ægophony, applied by Laennec to vocal signs, are still, and will probably always remain, in common use. It is, however, conceded on every side that in the description and interpretation of the first two of these signs (bronchophony and pectoriloquy) Laennec's treatise is defective. It is evident, in reading that portion of his treatise which is devoted to these signs, that his mind was biased by the desire to establish pectoriloquy as a cavernous sign. According to his description of bronchophony, "the voice seldom traverses the stethoscope." In pectoriloquy, on the other hand, the voice traverses the stethoscope either wholly or in part. By this language he means, doubtless, that the voice in pectoriloquy seems to be more or less near the ear of the auscultator. The distinction is a just and good one, but, as will presently be seen, it is not characteristic of a cavernous sign. The confused idea in Laennec's own mind is shown by his division of pectoriloquy into three varieties, namely, perfect, imperfect, and doubtful. The last variety, as he admits, is not to be distinguished by its intrinsic characters from bronchophony. A doubtful physical sign can hardly have much clinical value, and, quoting from an essay by Oliver Wendell Holmes, "To speak of the tones of the voice being heard a *short distance up the stethoscope*, is to present to the student a distinction of such tenuity as must seem beyond the reach of his faculties."[1] The vulnerability of this part of Laennec's treatise did not, of course, escape the critical eye of Skoda; but, if it be correct to say there is confusion in the account of bronchophony and pectoriloquy by Laennec, there is "confusion worse confounded" in Skoda's description. Skoda, after distinguishing the normal variations of the thoracic voice by the terms, *loud, clear*, and *humming* (terms which are not very sharply distinctive), and after concluding that bronchophony and pectoriloquy are identical, makes the following abnormal varieties: "1. The voice, accompanied by a concussion in the ear, completely traverses the stethoscope—loud bronchophony, which may be either clear or dull. 2. The voice, unaccompanied by concussion in the ear, passes incompletely through the stethoscope—weak bronchophony. 3. An indistinct humming, with or without a barely appreciable concussion in the ear."

[1] *Vide* Prize Dissertation. Published by order of the Massachusetts Medical Society, Boston, 1836.

It will be observed that there is no provision for ascertaining these modifications by immediate auscultation—the stethoscope is essential. I can sympathize with the student or the practitioner who attempts to grasp the distinctions set forth in this quotation, and to apply them at the bedside. The perplexity is not diminished by reading all that the author has to say with a view to their elucidation in the pages which follow. Adopting Skoda's account of vocal signs, it is not to be wondered at that a late German author, to whom I have repeatedly referred—Eichhorst—thus introduces a chapter on auscultation of the voice: "Its diagnostic value has been much over-estimated. It hardly ever furnishes original diagnostic results, and, almost without an exception, its object is to confirm conclusions obtained by previous methods of investigation." I am sure that this quotation does not express the estimate in which the vocal signs are held in our country, as bearing on the diagnosis of pulmonary diseases.

How are we to determine the signs which are incident to the loud voice and their differential characters? There is but one way, and that is by means of the analytical method of study. This method requires, as the point of departure for determining the morbid signs, study of the thoracic voice in health. This study shows that if we except characters which in some healthy persons belong to the voice as transmitted over the extra-pulmonary bronchi, especially on the right side of the chest, the *normal vocal resonance* is low in pitch, the voice seems diffused and distant from the ear, and it is accompanied by a perception of more or less vibration, thrill, or fremitus, which is not an acoustic but a tactile sensation. It is important to discriminate the fremitus from the resonance.

BRONCHOPHONY.

Proceeding now to morbid signs as determined by analytical study, it is a highly distinctive abnormal deviation from the lowness in pitch, the distance and the diffusion of the normal vocal resonance, when the thoracic voice is raised in pitch, seemingly concentrated and near the ear. These characters are present when the voice is transmitted through solidified lung. Calling the sign distinguished by these characters bronchophony, it always denotes a certain degree of solidification of lung, if we except the so-called normal bronchophony sometimes found on the right side of the chest, and in rare instances on the left side, at the summit, over the extra-pulmonary bronchi in healthy persons. The abnormal sign is rarely wanting if the requisite degree of solidification of lung exist. It is not necessary, for the production of this sign, that the solidification be complete. It is obtained in pneumonia and other affections which involve solidification of lung, when the associated respiratory sign is not the bronchial, but the broncho vesicular respiration, and, of course, the normal bronchophony is heard over lung not solidified, its production being evidently due to the proximity of the large bronchial tubes. Observe that intensity of sound is not included among the distinctive characters of bronchophony. The voice may be either loud or weak. If the voice be concentrated, near the ear, and high in pitch, no matter how feeble the sound, it is not less bronchophony than if the sound were ever so loud, and the diagnostic significance is the same.

INCREASED VOCAL RESONANCE.

The vocal resonance may be abnormally loud, with no marked alteration either in concentration, nearness to the ear, or pitch. A simple increase of the resonance, therefore, it remaining low, distant, and diffused, is a vocal sign distinct from bronchophony, and which, as clinical observations in connection with examina-

tions after death show, represents solidification of lung. The solidification thus represented is not sufficient in degree to give rise to bronchophony. Increased vocal resonance, as distinct from bronchophony, is also a cavernous sign. This statement is based exclusively on my own observations. If a cavity of considerable size be situated near the superficies of the lung, and not surrounded by solidified pulmonary structure, the vocal resonance is notably increased, and may be extremely intense, without the characters which are distinctive of bronchophony. The sign is often associated with cavernous respiration, and but rarely with amphoric respiration, the latter sign requiring solidified lung around the tuberculous cavity.

PECTORILOQUY.

Pectoriloquy, from its derivation, signifies thoracic *speech* as distinguished from thoracic *voice*, bronchophony having the latter signification. Strange to say, Laennec, who introduced these terms, did not employ them in accordance with this etymological distinction. His description of pectoriloquy embraced only the transmission of the voice. The confusion which has always existed, and still exists, in the use of the terms pectoriloquy and bronchophony, is readily and completely done away with by the use of these terms in the true etymological sense of each. Bronchophony should be considered as meaning transmission of the voice, together with the characters which are distinctive of that sign. Pectoriloquy should be limited to the transmission of speech, that is, of articulated words, through the chest.

With this definition, Is pectoriloquy to be rejected as a superfluous sign, as is done by Skoda? By no means. There is no sign better individualized than this. It is easy to decide, in any instance, whether or not the speech, that is, articulated words, is perceived. There is one liability to error, namely, the auscultator may hear the words from the mouth of the patient, and fancy that he hears them through the chest. In order to avoid this error, if immediate auscultation be employed or an uniaural stethoscope, the ear which is not to receive the chest sounds should be effectually closed. This, by the way, is a precaution to be observed in listening to any of the vocal signs. To make assurance doubly sure as to the existence of pectoriloquy, the auscultator should not know beforehand the words which the patient speaks. I have been accustomed to use this precaution in giving practical lessons in auscultation, and I have been led to notice that some persons seize much better than others, words transmitted through the chest.

Is pectoriloquy a cavernous sign? It is, and it is not. Articulated words may be transmitted through a pulmonary cavity. It is then purely a cavernous sign. They may be transmitted through solidified lung; it is then, of course, a sign not of a cavity, but of solidified lung. Now, is it practicable to determine clinically, whether the sign denotes a cavity or not? I answer in the affirmative. The discrimination is easy. If the thoracic voice which accompanies the pectoriloquy have the characters which are distinctive of bronchophony, the transmission of speech is through solidified lung. The two signs, pectoriloquy and bronchophony, are conjointly present. If, on the other hand, the bronchophonic characters are wanting, and the pectoriloquy is accompanied by merely an increased intensity of the transmitted voice, the transmission of speech is through a pulmonary cavity. The two signs, pectoriloquy and increased vocal resonance, are then conjoined. There is, therefore, a cavernous pectoriloquy, and there is a bronchophonic pectoriloquy, the differential characters being well marked and easy of recognition.

ÆGOPHONY.

Ægophony claims but a few remarks, not meaning to imply that it is a doubtful sign, as regards either its distinctive characters or its significance, but because, owing to correlative signs, it might easily be dispensed with in diagnosis. In fact, now when the exploratory puncture of the chest is employed in diagnosis without reserve, the auscultatory signs of pleuritic effusion are of much less practical importance than heretofore. Recently cases have been reported and papers published to show that, by means of the transmission of the whispered voice, it may be determined whether liquid within the pleural space be purulent or serous; but why waste time in observations and discussions relative to this point of inquiry, when by means of a hypodermic syringe in less than a minute the character of the liquid can be ascertained demonstratively?

It is a curious fact that in Laennec's treatise more than one-sixth of the portion of the work occupied with the consideration of the physical signs obtained by auscultation, is devoted to ægophony. In the greater part of this space, however, he discusses the mechanism of the sign. No one has found fault with or improved upon his description of the sign, and the name is well applied to it in certain instances, although the cry of the goat is less familiar to the people of this country than to the citizens of Paris. I believe that Laennec's interpretation of the sign is the true one, without regard to his views of the mechanism. The sign denotes a certain amount of pleuritic effusion. From Skoda's statement that he has heard ægophony in cases of simple pneumonia, I infer that either the pneumonia was accompanied by the requisite amount of liquid within the pleural space, or that he confounded ægophony with bronchophony. Analytically studied, ægophony has the same characters as bronchophony, minus the nearness to the ear, added to which is the tremulous or bleating quality. The sound is more or less distant from the ear, and is rarely accompanied by fremitus. I believe the sound to be essentially a bronchophonic voice, rendered more or less distant, the fremitus suppressed, the other modifications being due to its transmission through a stratum of liquid. Hence, the physical conditions for its production, in a case of pleurisy, are, the presence of a certain quantity of serous or purulent effusion, and condensation of a portion of the compressed lung sufficient to give rise to bronchophony. The latter condition is likely to exist with a moderate amount of effused liquid when the upper third of the lung is adherent to the chest wall by either recently exuded lymph or old adhesions, and the compressing force of the liquid as a consequence is brought to bear on the lower two-thirds of the lung.

I have repeatedly endeavored to connect bronchophony with ægophony by placing between the chest and the stethoscope an India-rubber bag containing varying quantities of liquid, in a situation where the bronchophonic voice was marked, in cases of pneumonia. The pitch, of course, is that of ægophony, and the voice is distant, but the tremulous, nasal, or bleating characters of the sign are wanting. The modifications expressed by these terms, therefore, depend on conditions not embraced in this experiment. Laennec attributed these modifications to flattening of the larger bronchi by the pressure of liquid, together with movements of the latter caused by the vibration of the lung. If this explanation be not satisfactory, it is certain that none better has been proposed. A bag of liquid interposed between the larynx and the stethoscope, occasions none of the characters of ægophony, and for a good reason, to wit, laryngophony is not bronchophony. The vocal sound from the larynx is characterized by intensity, concussion, and fremitus,

without that elevation of pitch which is essential in order to render it bronchophonic.

Reverting to bronchophony and to increased vocal resonance, while the clinical significance of these important signs admits of no doubt, the mechanism of their production is open for discussion. Laennec considered that an essential element in the mechanism is a better conduction of sound by solidified than by healthy lung. Skoda's experiments prove the reverse of this. If the lungs be removed from the chest, after death from pneumonia, a lobe or more being completely solidified, and an assistant speak through a tube, the end of which is applied first to a solidified and afterward to a healthy lobe, it will be found on auscultation with the binaural stethoscope that the voice is conducted further by the healthy than by the solidified lung.[1] Other experiments referred to in my last lecture also show that lung containing more or less air within the alveoli conducts sound better than either solidified lung or portions of the liver. The same result obtains if the voice of the assistant be directed into the larynx or trachea, and the experimenter auscultate in alternation a healthy and solidified lung. Laennec's explanation of the mechanism is, therefore, not tenable. To meet the difficulty, Skoda resorted to the theory of consonance. The adequateness of this explanation has been sufficiently disproved, and I need not take any time for the discussion of it. Here, then, is an apparent incongruity between an acoustic fact and clinical experience. We must admit that healthy lung conducts sound better than solidified lung. *Per contra*, it cannot be doubted that the thoracic voice, in cases of disease, is often louder over solidified than over healthy lung. Observations during life conjoined with examinations after death prove this incontestably. Which, then, is to give way, the acoustic fact or clinical experience? I answer, neither the one nor the other is to give way. The incongruity is, if possible, to be removed. The acoustic fact is not, for an instant, to be considered as invalidating the significance of the signs. Their significance rests on clinical and autopsical data which are not to be put aside in consequence of any apparent antagonism by physical facts. It is certain that such an antagonism must be only apparent, not real, and the object of inquiry should be to reconcile the apparently antagonistic facts with the truths of clinical experience.

It is to be considered that we cannot artificially combine all the physical conditions involved in the thoracic voice of either disease or health, nor even by means of the lungs removed from the body. The difficulties connected with artificial respiration are such that this method of experimentation is entirely out of the question; but something can be done toward a reproduction of the vocal signs by transmitting the voice through the larynx or trachea into the lungs after death. The contrast in the degree of conduction when the voice is transmitted through the trachea or a primary bronchus into a solidified lobe, with the transmission when the end of the speaking tube is placed upon the surface of the same lobe, proves that in the former instance the sound is conducted, not exclusively by the lung substance, but by air within the pulmonary bronchi. The agency of air within the bronchi is also demonstrated by the following simple experiment: Insert the end of the speaking tube into the trachea, the lungs remaining attached, removed from the body and artificially inflated. The lungs of the calf or sheep will answer as well as those from the human subject. An assistant speaks into the tube, and the vocal resonance over the lung is obtained by means of the binaural stethoscope.

[1] Holden's resonator makes an excellent speaking tube in these experiments.

Now, introduce a plug of cotton-wadding into the trachea, and compare the vocal resonance with that produced when the trachea is not thus obstructed. The intensity of the resonance is diminished, at least one-half, by plugging the trachea. The question, therefore, as to solidified or healthy lung being a better conductor of sound, has really not very much to do with the explanation of the well-established clinical truth that when the lung is solidified, often but not invariably, the thoracic voice is abnormally intensified. The explanation of this fact has more to do with the air contained within the bronchial tubes than with the solidification of lung. It is easy to account for the transmission of voice sounds from the larynx or trachea, as well as of the laryngeal and tracheal respiratory sounds, if we assume that a column of air extends into the bronchi within the lungs. We have only to instance the stethoscope and the speaking tubes in our houses, for familiar illustrations of the conduction of the voice by the medium of air contained in tubes.

The distinctive characters of bronchophony can be reproduced after death over solidified lung by speaking into the trachea. The voice, transmitted through a solidified lobe, the binaural stethoscope being applied to the lobe, is raised in pitch and near the ear, whereas it is diffused and comparatively distant when transmitted through a healthy lobe. An effect of solidification is thus to modify the vocal resonance as in life. I do not attempt to give an explanation of the manner in which these modifications are produced. The intensity of the thoracic voice, transmitted after death through a solidified lobe, is not increased; but increased intensity, as has been seen, is not to be considered an essential element of bronchophony. The bronchophonic voice may, or may not, be louder than the normal vocal resonance. But we have to inquire, why is it that bronchophony is ever more intense than the normal thoracic voice, and how is to be explained an abnormal increase of vocal resonance without the bronchophonic modifications whenever this is a sign of partial solidification of lung? The *rationale* must have to do with the air contained within the bronchial tubes. I submit the following explanation: Suppose a case of pneumonia in the second stage, one lobe, at least, being completely solidified. The solidified lobe is enlarged in volume nearly or quite to the limit of the expansion at the end of the inspiratory act. Its volume does not diminish with expiration, but remains the same in the inspiratory and the expiratory act. The respiratory movements on the affected side are more or less restricted. Now, the voice is produced by the breath in the act of expiration, that is, it is produced when the current of air is passing from the bronchial tubes of the healthy lung, in consequence of the diminution of the volume of this lung by the contraction of the thoracic space. It seems fair to assume that, under these circumstances, the bronchial tubes on the affected side contain more air than on the healthy side, and that there is less of a current of air in the expiratory act toward the trachea and larynx. Do not these points of difference account for a better conduction of the voice by air within the bronchial tubes on the affected side?

The following experiment has some bearing on this question: A hospital patient under my observation, died with acute pneumonia, the physical signs during life showing solidification of the entire left lung. In order to compare the transmission of the voice before and after the removal of the contents of the chest, the larynx was exposed and within it the end of the speaking tube inserted, the chest walls remaining intact. Blowing into the tube with the bellows and with the mouth produced over the upper solidified lobe a high-pitched tubular sound, and over the right upper lobe a

vesicular murmur. Moist bronchial râles were heard on both sides, but their presence did not drown the tubular and the vesicular sounds. Speaking through the tube and auscultating with the binaural stethoscope, well-marked bronchophony was heard at the upper part of the chest on the left side, over the solidified lung, and on the healthy side, in the same situation, a resonance corresponding to the normal. The bronchophony, although well marked, was much less intense than during life. Exposing now the lungs, by removing the sternum, and applying the stethoscope upon the upper solidified lobe, bronchophony was still heard, but notably less marked than before the chest was opened. I infer from this comparison that the bronchi within the solidified lung when *in situ*, the chest unopened, contained more air than when exposed directly to atmospheric pressure. But in this experiment only one of the physical conditions underlying the thoracic voice in life is represented. The production of the voice with the current of air in expiration, and the thoracic respiratory movements are, of course, not included.

Granting that the explanation which has been given of an increased intensity of vocal resonance over solidified lobe or lobes in cases of pneumonia be satisfactory, it remains to endeavor to explain an increased intensity of the thoracic voice when the solidification is not sufficient to give rise to bronchophony; when, for example, in cases of phthisis, the increased resonance of the voice represents a greater or less number of tuberculous nodules. Here it cannot be said, in explanation, that the lung remains permanently expanded, nor that the movements of the chest are notably restricted. The bronchial tubes in the vicinity of the nodules may be dilated, but I am not prepared to state that this is the rule. It is intelligible that these tubes may be prevented from collapsing by the proximity of solidified nodules. These physical conditions, however, seem hardly adequate to explain the sign. It is still more difficult to explain increase of vocal resonance when portions of lung are partially condensed by the pressure of liquid or an extrinsic tumor. There must be physical conditions involved which, as yet, are not understood. But, I repeat, to accept this conclusion is not to throw a shadow of doubt upon the diagnostic value of increased vocal resonance as a sign of solidification of lung, wherever associated with other signs denoting that condition.

The increase of vocal resonance over cavities is sufficiently intelligible. If a cavity be superficially seated; if it be not surrounded with lung completely or considerably solidified; if it be empty, and there be free communications with unobstructed bronchial tubes, we have a combination of physical conditions favoring the conduction of the voice, so as to give rise to more or less intensity of resonance, with fremitus, but without the modifications of bronchophony. In a lung removed from the body, this combination of physical conditions existing, notable intensity of vocal resonance, as distinct from bronchophony, may be produced by transmitting the voice into the trachea.

It did not occur to Laennec to study the thoracic sounds produced by the whispered voice. Many years ago, impressed with their value, I was led to institute a series of signs derived from this source. The sounds thus produced have, of course, characters corresponding to those belonging to the expiratory act in respiration; but they are brought out in stronger relief, and are better observed, in connection with whispered words. As these signs are correlative to those produced by the loud voice, it seemed appropriate to designate them by corresponding names. Whispering pectoriloquy was a term which had already been used. Applying to the sounds in health the name *normal bronchial whisper*, the morbid signs were named as follows: *Increased bronchial whisper*, *bronchophonic whisper*, *cavernous whisper*, *whispering pectoriloquy*, and *amphoric whisper*. The differential characters, following the same order in enumeration, are those of the expiratory sounds in broncho-vesicular respiration, bronchial respiration, cavernous respiration, pectoriloquy with the loud voice, and the amphoric voice. It would be superfluous, and therefore tedious, to enter into further description of these signs, and to consider more fully their significance. I would remark, however, that an abnormal increase of the bronchial whisper has often, in my experience, been of much service as one of the signs of incipient phthisis. But with reference to its significance in the diagnosis of that disease, it is essential to take into account the normal points of disparity between the two sides of the chest at the summit. In the infra-clavicular and in the interscapular regions, the normal whisper on the right side is louder than on the left side, but the pitch is a little higher on the left side. Consequently, if the whisper on the left side be louder than that on the right, or even equally loud, it is abnormal; and if the whisper be higher in pitch on the right side, it is abnormal.

Another practical remark relates to whispering pectoriloquy. Here, as with the loud voice, articulated words may be transmitted through solidified lung as well as through a cavity. Here, too, it is not less easy to determine in any instance whether the transmission be through solidified lung or through a cavity. If the pectoriloquy be associated with the characters of the bronchophonic whisper, it is the sign of solidification; but it is the sign of a cavity when associated with the characters of the cavernous whisper, that is, if it be low in pitch and non-tubular in quality.

CONCLUDING REMARKS.

Mr. President and Members of the Philadelphia County Medical Society: In concluding these lectures, I beg to recall the objects which were stated at the outset. I was desirous of showing that Auscultation and Percussion, divested of theories, speculations, non-essential discussions, and needless refinements, may be so simplified as to be made generally available in medical practice. The number of signs obtained by these two methods of physical exploration need not much exceed thirty. In a considerable proportion the characters of these signs are so plainly distinguished, that even their names suffice for a description and their recognition. I desired to show that in order to secure definiteness and clearness as regards the distinctive characters of different signs, and a ready differentiation of them, they must be studied by means of the analytical method. I cannot too strongly express my sense of the importance of relying upon this method of study for our practical knowledge of the signs obtained by auscultation and percussion. I have submitted, as I hope with becoming modesty, the fruits (if I may venture to use that term) of my own studies in this field of medicine for many years—studies relating to the distinctive character of signs, their significance, the introduction of some new signs, and the names by which they are to be designated.

Not ignoring the interest belonging to inquiries concerning the mechanism of the signs, I have entered in these lectures somewhat into a consideration of them in this aspect. My purpose has been chiefly to show that most of the more important of the signs may be produced out of the body, either by simple artificial means, or by using for this end the lungs, healthy and diseased, from the human subject and from inferior animals. The attention which I have of late given to the experimental illustrations to which I have referred, has led me to appreciate, more than hitherto, their usefulness, not only as explanatory of the mechanism

of signs, but as affording valuable aid in teaching practically auscultation and percussion. The production of signs out of the body in the class-room, before bringing students to the bedside for the actual illustrations, must be of much service in facilitating the acquirement of practical knowledge of their distinctive characters, and I commend this exercise to the consideration of instructors in physical exploration.

I need not remind this audience of the inestimable value of the results of the inspired thought which prompted Laennec to improvise a stethoscope by rolling together a few quires of paper. Have these results, after half a century, reached their termination? Is nothing to be expected in the future, in the way of improved means of auscultation? Does the binaural instrument represent the perfection of stethoscopy? I trow not. When we reflect upon the recent development in practical acoustics, as exemplified by the telephone, the microphone, and the phonograph, may we not expect that some inventive genius will develop similar marvels in auscultation? Long before the time of Laennec, a quaint English writer uttered these *quasi* prophetic words: "Who knows but that one may discover the works performed in the several offices and shops of a man's body by the sounds they make, and thereby discover what instrument or engine is out of order." This prediction has been fulfilled. And now, after the manner of Robert Hook, who wrote in 1705, who knows but that the time may come, and perhaps ere long, when the only use of the stethoscope of to-day will be to illustrate, by contrast, an immense improvement in the means of discovering "the works performed in the several offices and shops of a man's body by the sounds they make?"

Treating, as I have done in these lectures, of auscultation and percussion in a purely didactic fashion, I have had to deal with dry topics, which must have been dull to those who have not felt any special interest in the subject. This was unavoidable. The consciousness of the fact enhances the thanks which, under any circumstance, would be due to those who have honored me with their attendance and attention. And in closing, as at the beginning, let me say that I could not adequately express my sense of the honor involved in the invitation to inaugurate a plan of annual lectures which, as I hope, will hereafter be committed to abler hands.

ORIGINAL ARTICLES.

THE MICROCOCCUS OF GONORRHŒAL PUS—INFECTIVE VIRULENCE NOT DUE TO THE PRESENCE OF THIS PARASITIC MICRO-ORGANISM.

BY GEO. M. STERNBERG, M.D.,
SURGEON, U. S. A.

MODERN scientific research having demonstrated that a number of infectious diseases are due to the presence in the infective material—blood, etc.—and multiplication in the bodies of infected animals, of parasitic micro-organisms, having definite and distinct—*specific*—characters, it is not surprising that many physicians are inclined to make the generalization that all infectious diseases are parasitic, and it must be admitted that there is much to be said in favor of this hypothesis. A proper scientific conservatism, however, requires that the list shall be considerably extended before such a generalization can be considered safe; and it is evident that if a single infectious disease is shown to be independent of all micro-organisms, the generalization will be impossible, and the etiology of each specific disease of this class must be worked out separately.

But such a demonstration cannot rest upon negative evidence, for no one will venture to claim that failure to discover parasitic organisms in animal tissues and fluids is sufficient proof of their non-existence, especially since Koch's discovery of a bacillus in material which had previously been examined by numerous competent microscopists without success, at least so far as the demonstration of this particular organism is concerned.

On the other hand, a positive demonstration that infective virulence is due to something else than a living ferment, as, for example, to a peculiar albuminoid product elaborated in the body of an infected animal, would make the hypothesis of an undiscovered "formed ferment"—living organism—unnecessary and unreasonable; for we do not require two efficient causes to enable us to account for the same phenomenon. But I am not aware that such demonstration has been made in any one of the infectious diseases, and I have no evidence of this kind to offer in the case of gonorrhœa.

The question, then, as to the cause of the virulence of gonorrhœal pus remains exactly where I found it when I commenced the experimental study recorded in the following pages, for my researches show that—

The micrococcus which I have found in a certain number of the pus-cells in every specimen of gonorrhœal pus examined by me is an accidental parasite which has nothing to do with the special virulence of this fluid. And—*A careful search with a first-class objective* (Zeiss' 1–18 in.) *and by the use of staining reagents* (methyl violet and other aniline colors) *has not revealed the presence in this fluid of any other micro-organism than this micrococcus.*

This being the case, it may be thought that there is nothing more to be said. But it has seemed to me that a somewhat extended account of the experiments which have led me to the above-mentioned result may be useful in the present stage of inquiry (*a*) to substantiate the statement made, (*b*) to clear the way for others who may undertake a like investigation, and (*c*) more especially as a contribution to the study of micrococci.

My researches show that differences, morphological and physiological, exist among these lowly plants which make it possible to establish species, having characters quite as well defined as have many plants much higher in the scale.

This is in accordance with the views of Cohn and of Koch, and opposed to those of Billroth, Nägeli, and others, who consider all the bacteria as constituting a single species (Billroth), or at least but a small number of species, each including a variety of forms, designated micrococcus, bacterium, vibrio, etc. (Nägeli), which pass constantly the one into the other, being merely different phases in the life-history of the same plant.

That the life-history of some of the bacteria does include a variety of forms can not be questioned. Thus we have in the case of *bacillus anthracis* short rods, in the blood of an infected animal, which

grow into long filaments, in a culture-fluid, in the interior of which are developed oval spores, which are afterwards set free, and might be mistaken for micrococci, but for the fact that they again develop into rods. The life-history of other species also includes a motile form. But there is nothing in such facts to preclude the idea that a large number of distinct species exist in nature, and a somewhat extended study has convinced me that such is the case. Many of these species have, possibly, been independent of each other, and rivals in the struggle for existence, from the earliest period of the earth's history. But as among higher plants, these lowly organisms are subject to variations as to size, rapidity of development, etc., due to circumstances relating to their environment—abundance and kind of pabulum, temperature, etc.; and we already have ample proof that varieties possessing different physiological characters may be produced by special methods of cultivation—attenuation of virus in anthrax and fowl-cholera (Pasteur). The fact that well-defined species do exist, even among the smallest and simplest of the bacteria, will be enforced in the present paper by contrasting two species of micrococcus, which the writer has had under almost daily observation for many months past, and which have been cultivated side by side, in hermetically sealed flasks,[1] without even seeing one form pass into the other, or either develop into anything else than a micrococcus.

One of these species is the micrococcus of gonorrhœal pus, which turns out to be *Micrococcus ureæ*, Cohn, and which has been shown to be the cause of the alkaline fermentation of urine (Pasteur).

The other is the micrococcus found in human saliva, which has been proved to be a pathogenetic species so far as rabbits are concerned.[2]

In my paper upon a fatal form of septicæmia in the rabbit, published in *Studies from Biological Laboratory Johns Hopkins University*, I devote considerable space to the morphology of the septic micrococcus which is also illustrated by photo-micrographs (heliotype prints); a photo-micrograph of the micrococcus of gonorrhœal pus is also introduced, and at the close of this paper is referred to in the following words.

"Fig. 5 is introduced to show that there are micrococci and *micrococci*. The species (?) here represented was obtained in the first instance from gonorrhœal pus. A little of this pus, obtained from a case of two weeks' duration, showed upon microscopical examination, in a few of the pus corpuscles, an invasion by micrococci, while the majority of the corpuscles, as well as the liquid in which they were suspended, were free from organisms. A culture-tube containing sterilized *bouillon* (from rabbit), was inoculated with a little of this pus, and an abundant development of micrococcus resulted. A second tube was inoculated from the first, and a third from the second. The organism was found in abundance in all of these solutions (kept in a culture-oven at 37° Cent.), unchanged in appearance and unmixed with any other forms of bacteria. One cubic centimetre of the liquid from culture No. 3, was injected under the skin of a small rabbit with an entirely negative result. It is evident, then, that physiologically, this micrococcus differs from the deadly septic micrococcus which we have been studying. . . .

The question will naturally be asked as to the possible relation of this organism to the peculiar virulence of gonorrhœal pus. I have not yet found time to study this question experimentally, but think it quite probable that this organism will be found to be identical with the micrococcus found in pus from other sources, *e. g.*, open wounds, inflamed mucous membranes, etc. Whether this common and widely distributed micrococcus is capable under special conditions of cultivation of developing into various pathogenetic micrococci; whether it is a distinct species from our septic micrococcus, or whether the latter is a pathogenetic variety developed from it, are questions which can only be settled by extended and painstaking experimental investigations."

The questions above suggested, I have since attempted to settle by the experimental method, and the object of the present paper is to place upon record the results of my researches.

These results may be summarized as follows:

(*a*) Microscopical examination of numerous specimens of gonorrhœal pus, from a number of cases, has demonstrated the constant presence of the micrococcus first observed in Baltimore in the summer of 1881.

(*b*) Upon inoculating a sterilized culture-fluid (beef-tea, or rabbit *bouillon*) with a small quantity of gonorrhœal pus at the moment of its escape from the meatus urinarius, and placing this in a culture-oven for a few hours, an abundant development of micrococci takes place.

(*c*) These micrococci present in all cases the same morphological characters, and no other micro-organisms than these make their appearance in a hermetically sealed culture-flask,[1] inoculated as above indicated.

(*d*) These micrococci, introduced beneath the skin of a rabbit, produce no noticeable result.

(*e*) Culture-fluids containing these micrococci introduced into the healthy male urethra do not give rise to specific urethritis, or to any other noticeable result.

(*f*) A culture-fluid containing this micrococcus, upon being added to acid urine recently passed and free from micro-organisms, causes it to undergo the alkaline fermentation.

(*g*) Urine which has spontaneously—that is, without resort to experimental inoculation—undergone alkaline fermentation, contains a micrococcus identical in appearance with that obtained by cultivation from gonorrhœal pus.

The deduction from these facts is evident. It has already been formulated, and is included in the title of the present paper.

[1] *Vide* paper in Studies from Biological Laboratory Johns Hopkins University, vol. ii., No. 2, p. 164, for methods employed.

[2] *Vide* papers by the present writer, National Board of Health Bulletin, April 30, 1881; also Studies from Biological Laboratory Johns Hopkins University, vol. ii., No. 2, p. 182; Philadelphia Medical Times, September 9, 1882; American Journal of the Medical Sciences, July, 1882, p. 69-76; Philadelphia Medical Times, 1882; also a paper by Dr. Claxton, Philadelphia Medical Times, July 17, 1882, p. 627.

[1] The writer's method of conducting culture-experiments is described in vol. ii., No. 2, of Studies from the Biological Laboratory Johns Hopkins University, p. 164.

RECORD OF EXPERIMENTS.

At the outset of the investigation the idea naturally suggested itself that the question as to the possible causal relation of the micrococcus to the infective virulence of the fluid containing it, could best be answered by culture-experiments, carried far enough to exclude everything but the living organism, and by inoculation experiments upon susceptible animals.

The following experiments were therefore made with a view to ascertain whether the animals most easily obtainable for such a purpose were susceptible to the action of the virus.

Experiment No. 1 (May 6th).—Inoculated dog (male) in urethra and in right eye with gonorrhœal pus obtained from urethra of a patient in the post hospital, Fort Point San José. In this and the following experiments the pus was obtained from the urethra of the patient by means of a silver probe having a little cotton twisted about its extremity. This was then introduced into the urethra, conjunctival sac, or vagina of the animal experimented upon, and rotated vigorously to make sure that a portion of the pus was detached.

Result of above experiment entirely negative.

Experiment No. 2 (May 8th).—Inoculated female dog in right eye and vagina from same patient as in preceding experiment.

Result negative.

Experiment No. 3 (July 28th).—Inoculated puppy, two months old, in right eye and in vagina with gonorrhœal pus from a patient in post hospital.

Result negative.

Other experiments were made upon rabbits, but the notes have been mislaid. None of these experiments afforded any encouragement to the idea that the test could be made upon the lower animals, and I accordingly determined to seek an opportunity to make it upon man. My first efforts, by the offer of a bribe, to find a willing subject, were unsuccessful, but fortunately my friend, Prof. Hirschfelder, came to the rescue and offered to introduce my culture-fluids into the urethræ of certain patients in his wards in the San Francisco City and County Hospital. These patients consented to the operation with a full knowledge of the possible results, from a desire to please their doctor, and under the promise of speedy cure and a suitable recompense in case of successful inoculation.

The pus from which the cultures used in these experiments were started was taken from cases in the acute stage of the disease, and which had not been subjected to any local treatment.

Experiment No. 4 (July, 1882).—Made by Dr. Hirschfelder with material furnished by the writer. A culture-fluid, fifteenth, containing the micrococcus of gonorrhœal pus was introduced into the urethræ of three patients in the city and county hospital, upon small wads of cotton which were thoroughly moistened with the fluid and left *in situ* for fifteen minutes.

Case 1, J. D., has been in bed for about nine months; caries of the vertebræ.

Case 2, J. B., colored; syphilitic paralysis.

Case 3, D. M., in bed some time; aneurism of the abdominal aorta.

The result was entirely negative.

Experiment No. 5 (Aug.).—A fresh culture, fourteenth, from another, and recent case, was introduced in the same manner into the urethra of J. D., subject of previous experiment.

Result negative.

The culture-fluid, rabbit *bouillon*, used in the above experiments was neutral. In the following a slightly alkaline culture-fluid was used.

Experiment No. 6 (Aug.).—A fresh culture, thirteenth, was introduced into the urethra of W. B.

Result negative.

As already stated, the injection, in Baltimore, of one cubic centimetre of the third culture from gonorrhœal pus produced no perceptible result. This experiment I have since several times repeated.

Experiment No. 7 (June 10th).—Injected thirty minims of sixth culture from gonorrhœal pus beneath the skin of a small rabbit.

Result negative.

Experiment No. 8 (July 17th).—Injected thirty minims of second culture from gonorrhœal pus into subcutaneous connective tissue of small rabbit.

No result.

Experiment No. 9 (July 26th).—Injected ten minims of *sixth culture from blood of septicæmic rabbit* containing the septic micrococcus (Fig. 6) beneath the skin of the rabbit used in last experiment (No. 8). The animal died at 10 A. M., July 29th, and a microscopical examination made at once demonstrated the presence in the blood, and in the effused serum in subcutaneous connective tissue of the septic micrococcus in great abundance.

Remarks.—This experiment shows very plainly the difference between a pathogenetic micrococcus and one which is harmless. These organisms, although having distinct morphological characters (see Figs. 2 and 6), do not differ greatly in size, and both multiply freely in the culture-fluid used. This fluid, with the contained micrococci, proved to be innocuous in the dose of thirty minims in the one case, while one-third of this amount kills the same animal by reason of the presence of the other micrococcus in a subsequent experiment.

Experiment No. 10 (July 29th).—One drop of gonorrhœal pus from a recent case was introduced beneath the skin of a small rabbit.

Result negative.

This experiment is given for what it is worth. I have not yet had the opportunity to repeat it with a larger quantity of material.

The possibility that the micrococcus from gonorrhœal pus might acquire pathogenetic properties by being cultivated in blood-serum having occurred to me, the following experiment was made.

Experiment No. 11 (September 15th).—Injected ten minims of culture of micrococcus from gonorrhœal pus in blood-serum beneath the skin of a guinea-pig.

Result negative.

Remarks.—A single experiment of this kind has of course but little value, and I have not yet found time to follow up this line of inquiry. The question is however an important one, and well worthy of the attention of experimenters in this line of investigation. Here is a micrococcus which is harmless under ordinary circumstances. May it develop pathogenetic properties as the result of special conditions as to temperature, pabulum, etc., artificially maintained for a length of time, and in the course

of successive generations produce a physiological variety having different vital reactions from those which marked the parent stock?

To test the question as to a possible local pathogenetic action of this micrococcus in an open wound, the following experiment was made.

Experiment No. 12 (August 4th).—An incised wound was made with scissors, removing a fragment of skin, upon each thigh of a half-grown rabbit. The wound upon right thigh was moistened with a culture-fluid (twentieth culture) containing the micrococcus from gonorrhœal pus. The wounds were then dressed with dry tow and a bandage applied. Both healed kindly without any undue inflammation, and no difference was observed between the two.

Remarks.—In the recent work on *Antiseptic Surgery*, by Watson Cheyne, the statement is made that micrococci, which, from his illustrations, may well be the same as these, are often found beneath the dressing in the discharges from wounds treated antiseptically, and that no harm seems to result from the presence of these micro-organisms, all of which shows that there are micrococci and *micrococci*.[1]

Having determined that the micrococcus of gonorrhœal pus does not give to this fluid its specific virulence, and having observed its morphological identity with the micrococcus found in urine undergoing alkaline fermentation (*Micrococcus ureæ*, Cohn), the next question was as to the functional or physiological identity. The following experiments establish this:

Experiment No. 13 (July 26th).—Acid urine, drawn from the bladder with precautions to prevent contamination with micro-organisms located at orifice of the urethra,[2] was inoculated with the eighteenth culture from gonorrhœal pus. The urine, in a hermetically sealed flask, was placed in the culture-oven, and the following day was found to be pervaded with micrococci in pairs (Fig. 2), and to be highly alkaline.

Experiment No. 14 (September 3d).—Urine was passed into a test-tube, sterilized by heat, the first flow being rejected (as this contains micro-organisms washed from the mouth of the urethra, *vide* paper above referred to), and was then divided into three portions in three sterilized tubes.

No. 1 was inoculated with the thirtieth culture from gonorrhœal pus.

No. 2 was inoculated with the ninth culture from urine which had spontaneously "broken drown"—alkaline fermentation—and which contained *Micrococcus ureæ* in abundance.

No. 3 was retained for comparison—*témoin*.

The urine was transparent and acid. The external air was excluded from the test-tubes by covering each one with a bit of sheet rubber tied fast about the neck. The tubes were placed in the culture-oven at 100° Fahrenheit, and the following morning Nos. 1 and 2 were found to be clouded, to be pervaded by the micrococcus, and to have an alkaline reaction. No. 3 remained transparent and acid.

Remarks.—I have made many other experiments of the same kind as those above given, but these will suffice to show (*a*) that the micrococcus of gonorrhœal pus causes urine to undergo alkaline fermentation; and (*b*) that it does this by virtue of its own vital activity, and not as a mere carrier of a chemical ferment present, in the first instance, in the gonorrhœal pus used to inoculate culture No. 1, for this hypothesis would require that such a ferment should be as active when diluted to an incredible degree (thirtieth culture) as when present in an appreciable amount—*e. g.*, in the first culture.

In my method of conducting culture-experiments the amount of material taken from one culture-tube to inoculate the sterilized *bouillon* in a similar tube, for the succeeding culture, is less than the fiftieth part of the contained fluid. The amount of dilution to which the original material, introduced into culture No. 1, was subjected in the above experiment may therefore be estimated by raising 50 to the thirtieth power. It is hardly necessary to introduce the array of figures which would result from this computation in order to convince the reader that the original material is practically excluded, and that only the remote descendants of the living organisms which were present in it are likely to be found in our thirtieth culture.

I may mention here, incidentally, that I have ascertained in the course of my experiments that *Micrococcus ureæ* is not the only organism which has the power of transforming urea into carbonate of ammonium—$CH_4N_2O + 2H_2O = (N.H_4)CO_3$.[1]

(To be continued.)

[1] I have not the work referred to at hand, and consequently cannot give the exact reference, but believe that my memory is not at fault with reference to the statement made.

[2] *Vide* paper by writer in Studies from Biological Laboratory, *loc. cit.*, p. 177.

[1] Schutzenberger, Fermentation. D. Appleton & Co., New York, 1879, p. 203.

WHEN IS A PHYSICIAN LEGALLY EXEMPT FROM TESTIFYING TO CONFIDENTIAL COMMUNICATIONS MADE TO HIM BY HIS PATIENT?

BY FRANCIS W. SHAIN, M.D.,
OF PHILADELPHIA.

WHAT is a confidential communication or privileged communication in the eye of the law? It is a communication made *bonâ fide*, upon any subject-matter in which the party communicating has an interest, or in reference to which he has a duty, and as the occasion is an absolute privilege, the only questions are whether the occasion existed, and whether the matter complained of was pertinent to the occasion.

In a degree, the law extends this privilege to the lawyer, clergyman, and physician—but not equally, and this we would like to see remedied.

At common law such privileged communication extends only to the lawyer, and naturally, as the profession constantly brought into relation with the accused; it was conceded on the ground of public policy, because greater mischief would probably result from the requiring or permitting of such testimony than from wholly rejecting it; and further, as a fundamental right for the sole and exclusive protection of the client either in person or property, by whom and through whom the obligation can only be removed. It is made perpetual. Does not end or cease with the completion of any par-

ticular business, the seal of the law once fixed remains forever; and it is generally comprehensive, as implied in the language of the distinguished jurist, Lord Chancellor Brougham: "If the privilege were confined to communications connected with suits, begun, or intended, or expected, or apprehended, no one could safely adopt such precautions as might eventually render any proceedings successful, or all proceedings superfluous." To the lawyer, therefore, by the common law the privilege is universal.

What is the position of a clergyman in this respect? Is he or is he not exempt from testifying to communications made to him in his professional capacity? This question has been the subject of a warm debate on the ground of conscience and general good to society, in that the guilty conscience may unfold itself, and ask for spiritual advice and guidance, and to seek pardon and relief.

The law of Papal Rome has declared that such communications are privileged, and has gone so far as to punish her priests who reveal them, holding to her unchanging doctrine that the confessions are not made to the priest as a man, but in his professional character as the representative of the Deity. In Scotland, if a prisoner confesses his crimes to a clergyman in order to receive spiritual advice, the clergyman is not required to give such communications in evidence.

The English law encourages a penitent to confess his sins for the purpose of unloading his conscience; the clergyman is only exempt from presenting his penitent to the civil magistracy (that is not to turn State's evidence voluntarily) "under pain of irregularity," but must make a full and perfect statement of the communication or confession so received by him in his professional character, whenever required by the law so to do, as it made no distinction between clergymen and laymen. The citizens of New York, through their legislature, passed a statute which has forever, as far as they are concerned, set this vexed question at rest. It is as follows: "No minister of the gospel, or priest of any denomination whatsoever, shall he allowed to disclose any confessions made to him in his professional character, in the course of discipline enjoined by the rules or practice of such denomination." (2 *Revised Statutes*, page 406, § 72.) The Courts of New York have held to the principles of this statute whenever required or requested so to do. It is a pleasing fact to notice that the same protection has been accorded to penitents and clergymen in the following States: Missouri (*Revised Statute* 1845, c. 186, § 19; Wisconsin (*Revised Statute* 1849, c. 98, § 75); Michigan (*Revised Statute* 1846, c. 102, § 80); and in Iowa (*Code* 1851, art. 2393). In States other than those above mentioned the common law principles prevail.

In regard to the communications between a physician and his patient:

The physician under the common law has no status granting privileged communications, nor can the common law be subject to amendment; relief must therefore be looked for by statute law, granted by the legislatures of the respective States.

In several States such statutes now exist, almost rendering unnecessary an argument to prove the right and propriety of extending the privilege to physicians.

The Legislature of the State of New York comes to the relief of the citizen while living, and to his reputation when dead, by a statute of the following character, viz:

"No person, duly authorized to practise physic or surgery shall be allowed to disclose any information which he may have acquired in attending any patient in a professional character, and which information was necessary to enable him to prescribe for such patient as a physician, or to do any act for him as a surgeon." (*Revised Statutes* New York, vol. ii. page 406, § 73.)

The following named States have likewise passed statutes to the same effect: Missouri, Wisconsin, Michigan, and Iowa; but five States in the United States have conceded this wise privilege.

The Courts of New York have been called on for an interpretation of this statute in the only two ways in which such a statute could by any possibility bar justice.

The first was in the case of Hewett *vs.* Prime (21 Wend., page 79), where the Court held that a consultation as to the means of procuring abortion in another is not privileged by the statute; that if A should apply to a physician for a prescription for B who was pregnant, or as to the use of instruments or drugs on the person of B, such communication is not privileged, and the physician would be obliged to testify to the fact.

The second was in the case of Johnson *vs.* Johnson (14 Wend., page 637), in which the Court held the patient himself may waive this privilege, and the physician could then testify, as the privilege, said the chief-justice, was the patient's and not the physician's. He considered it analogous to an attorney and client in this respect; but no statute touching this question exists in Pennsylvania, and in bringing the subject to the attention of the physicians of this Commonwealth, it may be well to cite some illustrative cases, showing the importance to the citizen that he should be able to state the "whole truth" to his medical adviser without apprehension of self-commitment.

An erring and unfortunate citizen presents himself to his physician with a sore upon his genital organs or elsewhere, obscured by previous treatment; and, by a candid confession of exposure, he aids in the diagnosis of syphilis, and is at once properly treated; to his own great advantage, and to the protection of his wife or others from contamination, and so far restraining the spread of this loathsome and grave disease. Now, if this citizen was familiar with the fact that his medical attendant could be compelled in court to convict him of adultery or any other crime arising from illicit intercourse, he would hesitate to take counsel, to his own destruction and to the detriment of public policy.

Again, a man in attempting a burglary, is met by an excited and outraged opponent who strikes to kill, and receives a blow in the temporal region. There exists really a fracture of the bone and

laceration of the middle meningeal artery. The real condition is obscured by the swelling of the superficial tissues, and the patient knowing he must not commit his story of crime to the physician, and not feeling the gravity of the injury, tells the story of being struck by a snow-ball, or by a fall or some such slight cause; the physician is misled, and hours afterwards the patient dies from hemorrhagic compression of the brain. Had the whole trouble been told to the physician, knowing the provocation and probable force of the blow, his investigation would have been more thorough, he would have kept the patient under observation and been able to give him the hope of a trephining operation. Other cases suggest themselves, but these are sufficient to illustrate how necessary it may be for the patient to tell his story unreservedly and without fear to his medical adviser.

Such is the history of the law of the present day, and we trust its presentation will awaken the medical profession to take such action as may secure the passage of a similar statute in Pennsylvania, and indeed in every State in the Union.

HOSPITAL NOTES.

PENNSYLVANIA HOSPITAL.

(Service of DR. J. M. DA COSTA.)

A CASE OF DIABETES INSIPIDUS TREATED SUCCESSFULLY BY ERGOTIN.

(Reported by FRANK WOODBURY, M.D.)

THE following clinical note is worthy of record, not merely as furnishing an instance of the successful treatment of a disorder which had resisted many remedies during a long course of hospital treatment, but also because of the history of the acute beginning, and the progressively downward tendency of a purely functional (vaso-motor?) disease; unaccompanied, so far as could be ascertained, by organic lesions at any stage of its development. The treatment of diabetes insipidus by ergot is, of course, no longer a novelty; but it is still of interest to add an additional case to those already reported by Dr. Da Costa in THE NEWS (Jan. 7, 1882), where a discussion of the therapeutic method may be found.

Charles L., 28 years of age, born in Ireland, a farm laborer, was admitted August 14, 1882, into the ward under the charge of Dr. Arthur V. Meigs.

An interesting point in his family history which was elicited, was a possible hereditary tendency to renal disorder—his father had died of dropsy about three months before the patient's birth. His own health was usually good; he has served in the British army for three years; and had been in this country for eight years. He had spent the most of his time here in living on a farm, but had never had ague nor an attack of rheumatism. He acknowledged having had gonorrhœa, from which, however, he had entirely recovered; but he positively denied all other venereal infection. He never had received an injury or blow upon the head, nor a severe fall; he was equally positive on this point. In August, 1881, while in his ordinary health, he was working hard upon a farm, where he was exposed to cold and wet. About this time he stated that he had come into the city to buy a summer suit of clothing, which he wore on his return to the country; the clothes were thin, and on his way home the weather changed, and a cold rain commenced to fall, so that he became wet through before he reached there. The next day he felt as if he had taken a severe cold; he was dull and heavy; he had irregular chilly sensations and aching in his bones, which obliged him to remain in the house. Shortly after this, within the week, he noticed that he was passing more water, and that he had to rise several times in the night; he stated, however, that the aggregate quantity passed each day then, though more than normal, was not so great as it was a month later.

The malady continued; he urinated frequently, and was very thirsty all the time. In September, he was so ill that he was sent to the Philadelphia Hospital, where he remained under treatment for two months; then becoming dissatisfied, he left the institution, as he said, "worse than when he went in." He then sought admission into a hospital at Trenton, where for ten months he received medical care, but still he got worse rather than better, and as they had given him up to die, he left there, though very sick at the time and reduced almost to a skeleton. In this miserable condition he was found at the gate of the Pennsylvania Hospital about two weeks after leaving Trenton, and was taken at once into the ward. He was so very weak that, although he was passing eight pints of water *per diem*, it was thought better to give restoratives for a time before attempting to place him upon special treatment for diabetes insipidus. His urine, which was repeatedly examined, had a specific gravity of 1010, it was faintly acid in reaction, almost colorless, and was perfectly free from sugar and albumen. At the time the disorder was at its beginning, in September, 1881, he passed eight pints of urine, and again in January, 1882, he had passed a like quantity, as also he had after entering the ward, but he had never exceeded this to the best of his knowledge. Tincture of chloride of iron, quinine, and nux vomica, were given in succession for a short time, and with these, aided by a diet of milk and eggs and a small amount of stimulants, he revived considerably. In the course of a week after coming into the ward Dr. Arthur V. Meigs began the administration of ergot, of which twenty minims of the fluid extract were given three times a day. He took this small quantity for more than a month, but as it did not have any apparent effect upon the amount of the urine, it was stopped September 19th. No more ergot was given until October 17th, when the administration of ergotin hypodermically was begun; he at first received daily one injection, and afterwards two, of five grains (dissolved in fifteen minims of a solution of equal parts glycerine and water), therefore much larger doses than before. The effect of this treatment was promptly manifested. Under its use the urine had decreased to four pints by the end of the month; but on the remedy being then suspended for a few days, it increased again to six pints. He had greatly improved by October 30th. He had gained eleven pounds in weight in two weeks, his skin was not dry and harsh, he ate and slept well, and although he had been very thirsty before, he was much less so since he had been under the ergotin treatment. He still felt very weak, and was obliged to remain in bed, but his improvement was very manifest.

At this time the ward passed under the care of Dr. Da Costa, who decided to keep up the treatment, only giving the remedy by the mouth instead of hypodermically. The punctures had not given rise to abscesses, but the spots were tender and painful for a time after each injection.

On November 4th he had passed six pints during the preceding twenty-four hours. Five grains of ergotin, using the same solution, were now directed to be given by the mouth, morning and evening, and the succeed-

ing day the quantity of urine was again reduced to only four and a half pints. On November 6th the ergotin was directed to be given three times a day. The next day he was allowed to get out of bed; but as a result, he was not feeling so well on November 8th, and was passing five pints. As he complained of indigestion he was allowed to have a drachm of the compound tincture of gentian before meals, the ergotine being continued. After this he steadily improved: it was noted on the 13th, that "for the last three days the urine has measured three and a half pints daily." The tincture of cascarilla was now substituted for the gentian, as the later did not agree with his stomach. On the 16th, the man was brought before the class; he was still passing three and a half pints; the ergotine was now increased to four doses daily, his general strength and physical appearance had very greatly improved. On the 19th, the remedy was reduced one-half, and the next day discontinued entirely. The cascarilla was continued for a few days longer, but on the 24th strychnia (gr. $\frac{1}{60}$) was given in its place. The urine now ranged between three and three and a half pints daily, and he only occasionally had to rise at night. On the 21st of November, he took a long walk outside of the hospital, but there was no increase in the amount of the urine in consequence, and he slept better that night. As he was anxious to be discharged, he was detained but a few days longer under observation, and was allowed to go on the 30th, when he was apparently strong and well. While under the ergotin treatment he reported that he gained in six weeks the surprising amount of thirty-two pounds in body weight; he weighed in the middle of October but 126 pounds, when he was discharged he weighed in the same clothing 158 pounds, and appeared in sound, vigorous health. He considered that the treatment had saved his life.

MEDICAL PROGRESS.

THE TREATMENT OF PUERPERAL ECLAMPSIA WITH CHLORAL IN LARGE DOSES.—DR. GEORGE ROCHÉ reports a case of puerperal eclampsia occurring in a woman, aged 21, in the eighth month of pregnancy; the face was œdematous, and the urine loaded with albumen. The patient was first bled, cold compresses applied to the head, inhalations of chloroform and ether administered, and rectal injections of chloral in doses of 60 grains—in all 470 grains of chloral were given in 24 hours. Recovery took place.—*L' Union Méd.*, November 28, 1882.

FORTY INCHES OF BOWEL PASSED PER RECTUM, WITH RECOVERY.—At the meeting of the Baltimore Academy of Medicine, held December 5, 1882, Dr. CHRISTOPHER JOHNSTON exhibited a specimen of forty inches of intestine passed by a lady in Charles County, Maryland, per rectum. The patient was thirty-two years of age, married, and had one child about four years old; her health had always been delicate, and she had been subject to indigestion, constipation, and colic pains. The attack during which the present specimen was passed began with acute epigastric pain, at first supposed to be due to cramp colic. There had been no action from the bowels for six or seven days, and a circumscribed hardness could be easily felt through the abdominal parietes. After three days, stercoraceous vomiting set in, and continued three or four days; it was accompanied by some relief. At the end of the three or four days the sphacelated bowel, together with some of the omentum, was passed, followed by loose operations. Part of the specimen retained its tubular shape, and contained feces; another part passed mixed with feces. The prostration during the attack was very great, and death seemed imminent. She began to improve slowly, and to regain her appetite after the discharge of the bowel, but had still occasional pains in the bowels, especially in connection with her evacuations. Six weeks after the attack she was able to sit up a little, and though still feeble, was cheerful, and expressed herself as feeling very well. The treatment pursued was cathartics, enemata, inflation, anodynes, etc.—*Maryland Medical Journal*, January 1, 1883.

EXTRACTION OF A CATARACT WITHOUT EXCISION OF THE IRIS.—At the meeting of the Société de Chirurgie, held November 22d, M. GALEZOWSKI read a paper in which he urged the advantages of abandoning the incision of the iris in the extraction of cataract. He thinks that iridectomy does not prevent inflammatory accidents, and that it predisposes to secondary cataract. M. Galezowski advises making an ellipsoidal flap, making the incision at the edge of the sclerotic. In forty operations made without iridectomy since the month of July, he has had no ocular phlegmons. Iridectomy is, however, indispensable if the iris falls spontaneously under the knife, if there is posterior synechia, or if the border of the iris has been contused during the operation.—*Gaz. Hebdom.*, Dec. 1, 1882.

DISSEMINATED POLYPI OF COLON.—At the meeting of the London Pathological Society held Dec. 19, 1882, a specimen of diffused polypoid growth of the colon was shown by MR. BOWLBY. The case differed from others of the same kind in the absence of constriction of the intestine. The growths extended from the cæcum to the sigmoid flexure; the polypi had long pedicles, and consisted of fibrous tissue, covered by normal mucous membrane. He also showed a specimen of large polypoid growth of the rectum, removed by operation from a young woman; it was attached to the wall of the rectum, and was extruded from the anus during an effort at defecation; it had given rise to no symptoms. Mr. Bowlby also exhibited a specimen of polypus of the small intestine. The patient was a child, who suddenly experienced severe pain in the abdomen; when admitted into St. Bartholomew's Hospital the vermiform appendix and cæcum protruded as a gangrenous mass from the anus. The child died subsequently of congenital syphilis, and at the post-mortem examination it was found that there was some limited peritonitis of old standing, and a large polypus of the small intestine; the whole of the colon was absent, having apparently sloughed away; about three inches and a half above the anus was a constriction, which appeared to mark the point where the continuity of the gut was re-established.—*British Medical Journal*, December 23, 1882.

TREATMENT OF TYPHOID FEVER WITH CARBOLIZED CAMPHOR.—M. DUJARDIN-BEAUMETZ recommends the treatment of ataxic forms of typhoid fever with carbolized camphor. His mode of administration is by the rectum, in the form of injections, with 1 gramme of camphor to 5 decigrammes of crystallized carbolic acid, dissolved in 30 grammes of alcohol and 170 grammes of water. He claims the constant result of these drugs in low forms of typhoid fever has been a reduction of the fever and the disappearance of the nervous symptoms.—*Bull. Gén. de Thérap.*, November 30, 1882.

EXTIRPATION OF THE SPLEEN.—D. G. ZESAS extirpated the spleen in six rabbits under antiseptic precautions, and all recovered. The autopsies showed that there was in all cases an increase in size of the

liver and lymphatic glands, the increase being proportional to the time elapsing between the operation and the death of the animal. Two examinations of the blood, four and eight weeks after the extirpations, showed that at first the red corpuscles and then the white were diminished in number.—*Centralb. f. die med. Wissen.*, Dec. 2, 1882.

OXYGEN AND DISEASE GERMS.—Mr. F. J. Faraday calls attention in the *Times* to certain remarkable facts communicated by M. Pasteur to the recent Hygienic Congress at Geneva. Starting with the suggestion by Dr. William Roberts, F.R.S., that disease germs might be "spores" from harmless saprophytes which had acquired a parasitic habit, it has been argued that such "spores" might be developed by cultivation in the presence of noxious gases, or in confined places in which the proportion of free oxygen present in good air did not exist. The hypothesis has been specially applied to the evolution of the tubercle bacillus, but it is obvious that it is equally applicable to the evolution of the germs of other diseases, such as typhoid fever. The process described by M. Pasteur at Geneva, as having enabled him to convert the virus of the form of typhoid fever which caused great mortality among horses last year in Paris into its own vaccine, has a noteworthy bearing upon this hypothesis. M. Pasteur first tried to "attenuate" the virus by cultivating the specific microbe, which he had already discovered as associated with the disease, in contact with air. But experiments showed that the culture retained its fatal attributes for a certain period, when it suddenly became absolutely sterile, or, in other words, the microbe died. M. Pasteur then adopted a method which can only be described as a process of nursing the microbe, so as gradually to adapt it to a new mode of life, or, in other words, to modify it without destroying its fertility. Taking a virulent culture from the blood of a rabbit which, through inoculation, had died of the disease, he sowed fresh portions of this culture in veal broth on successive days, and kept the series in contact with air. He had thus a graduated series of cultures from virulent stock in process, each of these cultures having been subjected to the modifying influence of oxygen for a different period. M. Pasteur was thus able to seize the moment when the culture which had been exposed the longest to aeration became sterile, and to select a culture on the eve of sterility, which he transferred to a fresh infusion already found to be specially suitable to the microbes, and which consisted of two parts of veal broth with one part of pure rabbit's blood. Having, in fact, reduced the microbe to the verge of sterility, or death, he subjected it at this critical moment to an invigorating regimen, and thus protracted its vitality and made it the stock of a new series of cultures. By repeating this process again and again in all its details, M. Pasteur ultimately evolved a race capable of serving as the vaccine of the original virus, oxygen having been the modifying influence throughout.

There is a striking analogy between the treatment thus described and that by means of which, about six years ago, Fräulein Marie von Chauvin evolved *amblystoma*, a land salamander, from the water-breathing Mexican axolotl. She selected healthy animals, and first kept them in shallow water, so that they were not quite covered by the water. When their health declined she restored them to deep water. Gradually she accustomed them to shallow water, and eventually kept them on land in deep moss. She was obliged to force them to eat by compelling earth-worms to wriggle down their throats; and feeding them well at the critical stage of metamorphosis seems to have been the main condition of success. When the change from gills to lungs was perfected, they fed themselves with avidity. But Nature herself, notwithstanding the difficulties experienced by Fräulein von Chauvin in transforming her axolotl (some of which died under the treatment), apparently succeeds sometimes in evolving *amblystoma* from the Mexican newt; therefore, assuming the variability of specific microbes under the influence of oxygen, there is nothing unusual in the idea of the parasitic germs of epidemics being spontaneously evolved from harmless saprophytes under peculiar conditions of culture, such as the presence of various gases instead of free oxygen. In that case, ill-ventilated sewers, stagnant pools, and other places where aeration is not efficiently carried on, may continually evolve new crops of specific disease germs. Moreover, no embryologist will object to the attribution of the characteristics of species to the infinitely little, and the resistance to retransformation under artificial conditions displayed by M. Pasteur's typhoid microbe is only what we might expect, on the assumption that it is a confirmed new species. Fräulein von Chauvin's axolotls had to be forced to eat under the new conditions which she provided for them, and they would certainly have died if they had been left to themselves.

The behavior of M. Pasteur's typhoid microbe, which retained its specific or virulent character even under the influence of oxygen, until it suddenly died, is singularly like that of the axolotls. Does it not also explain the often sudden disappearance of epidemics? The typhoid germs, having acquired a parasitic or virulent habit, say in the sewer, are conveyed to the bodies of human beings. Though subsequently exposed even to the abundant oxygen of a healthy locality, they resist its influence for a certain period, spreading death meanwhile, and then, under the influence of sanitary conditions, they suddenly become sterile and the epidemic disappears.—*Knowledge*, October 13, 1882.

EXTIRPATION OF THE SUB-ORBITAL NERVE.—At the meeting of the Société de Chirurgie held Nov. 29, M. POZZI read a memoir by M. BLUM on this subject. A woman, aged 68 years, had for two weeks suffered from severe neuralgia of the left side of the face; all treatment proved unavailing, even the extraction of all the teeth of the upper jaw. M. Blum therefore cut down upon the sub-orbital nerve and tore it out; the pain then ceased, and has not returned. M. Pozzi states that this procedure has only been employed in three instances; one was followed by cure, and the other two were unsuccessful in relieving the pain.—*Gaz. Hebdomadaire*, Dec. 8, 1882.

SUBCUTANEOUS INJECTION OF ETHER IN IMMINENT DEATH FROM HEMORRHAGE.—At a meeting of the French Academy of Medicine, held December 19, 1882, DR. HAYEM read a paper with the above title, of which the following is an abstract. When a dog has been bled to the extent of causing convulsions, and death is imminent, injections of ether are of no service: transfusion of blood may, however, in such cases save the animal's life. The results of the subcutaneous injection of ether are also negative when an amount of blood equal to one-nineteenth the weight of the animal has been withdrawn and the animal brought into a condition where the chances for life or death are about equal. In this state also, life may be assured by transfusion or the injection of serum obtained from blood of an animal of the same species. These results show that transfusion cannot be replaced by the stimulant effects due to subcutaneous injection of ether, which only increases the number and vigor of the heart's pulsations, and does not augment the blood-pressure or rectal temperature.—*L'Union Médicale*, December 21, 1882.

THE MEDICAL NEWS.

A WEEKLY JOURNAL

OF MEDICAL SCIENCE.

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SATURDAY, JANUARY 20, 1883.

RESECTION OF THE TARSUS FOR INVETERATE CLUB-FOOT.

ESPECIALLY in the treatment of deformities, osseous surgery, if we may so designate it, has made rapid progress during the last ten years, and operations formerly unthought of, or deemed inadmissible, are now of frequent occurrence.

As a rule, club-foot is amenable to less heroic measures than bony resection, but occasionally cases occur that resist all other modes of treatment, and so long ago as 1853 Little proposed, and in 1854 Solly performed, an excision of the cuboid. Since 1874, however, chiefly owing to the operations of R. Davy and E. Reid, the systematic resection of portions of the tarsus have been undertaken. But while in England and Germany the operation has been known and accepted, in France and in this country but little has been done. Erskine Mason, Stephen Smith, Porter, Bradford, and other American surgeons, however, have done a few operations.

In the *Archives Générales*, we find an excellent paper on these operations by CHAUVEL, of Val-de-Grâce, which well repays perusal. Three different methods exist. In the first or "anterior," the cuboid only is removed; in the second or "posterior," chiefly or wholly the astragalus, and in the third, or "total cuneiform resections," a wedge-shaped piece is removed, involving chiefly the cuboid and its neighboring bones, but extending, if need be, even to the inner border of the foot, though in ankylosis Reid has applied it to the ankle proper.

The first operation has been done, according to the statistics of Chauvel, eight times on six patients. Recovery has followed in all cases. The immediate results have been good, but the ultimate results were satisfactory in only one-half of the cases.

The second operation was done thirteen times; six of these were resections of the astragalus and bones of the leg in wedge-shaped pieces, followed by six successful ankyloses; seven were resections of the astragalus, either alone or combined with removal of the external malleolus, followed by two definite successes, four cases in doubt, and one bad result.

The third method was practised thirty-seven times. Three deaths resulted, one on the tenth day from an old cardiac valvular insufficiency with recent endocarditis, one from hospital gangrene, and the third from septicæmia. The operation should be held responsible only in part for the first two. Of the ultimate results in the thirty-four cases that recovered, in twenty-four it was satisfactory, and in ten undetermined, some of which certainly would have been counted among the successes.

The deformities to which the operations are applicable are otherwise incurable varus, equino-varus, and equinus. In varus and equino-varus undoubtedly the total cunieform resection is the best operation, and has been oftenest done. Removal of the cuboid only has deservedly met with but little favor. If the case be one of pure varus, the wedge has its base at the cuboid, the width of the base and the length of the wedge being proportioned to the degree of the deformity to be corrected, and without special regard to the articular surfaces of the cuboid or other bones involved. Indeed it is less important that exactly this or that bone be removed than that the wedge be well placed, and of proper size. Not seldom indeed more than one subsequent slice has to be taken off the bone, in order to obtain good apposition of the bony surfaces. In equino-varus the excised portion must be wedge-shaped in two ways—(*a*) the base as before at the cuboid externally, and apex internally to correct the varus, and (*b*) the base above and apex towards the sole to correct the equinus. In equinus proper, the anterior operation or resection of the cuboid, and the total cuneiform method will not remedy the deformity so well as the posterior operation at the ankle-joint. This should consist not so much in the removal of the astragalus or of the astragalus and external malleolus, as of a wedge-shaped piece or pieces involving both the upper and lower articular surfaces of the ankle of such size and shape as to correct the deformity, or a wedge-shaped piece may be removed just in front of the joint involving the head and neck of the astragalus, and the calcaneum if necessary.

So far as possible these operations should be subperiosteal and antiseptic. Because they are deliberate and designed, they are none the less severe

compound fractures, involving also, in most cases, the neighboring articulations, and in the part of the body most distant from the centres of nutrition. Hence, every possible precaution must be taken to insure the least risk and the quickest healing. This at best, if we obtain bony union, must be slow, and cover some weeks or even months.

The operative procedure is simple. The incisions in ⌊ or ⊥ at suitable places and of suitable length give easy access to the bones. The saw may be used, but the chisel and bone-forceps are preferable, and in children a stout cartilage knife will often suffice. The arteries are well known and avoidable, and serious hemorrhage is rare. The dorsal tendons and, also, especially that of the peroneus longus should be carefully preserved. Tenotomy will be required occasionally, either at the time of the operation or subsequently. Antiseptic dressings and a suitable splint, with a foot-piece, are to be immediately applied. The splint in some form or other, according to the preference of the particular surgeon, is indispensable in order to retain the foot in its proper position.

If the results have been so good in these the earlier operations, we have every reason to believe that still better are in store for us within the next decade. American surgeons certainly should not be behind in working out so favorable a result.

BACILLUS LEPRÆ.

AMONG the infectious diseases which have recently been ascribed to a micro-organism is leprosy. A close examination of the studies which have from time to time been made upon the *bacillus lepræ* show a better foundation for such a view than would at first be expected, though the same examination will also show that the evidence in its favor is much weaker than in the case of the *bacillus malariæ*.

The first observations on this subject appear to have been made in 1873, by Dr. G. A. Hansen, of Bergen, Norway, and published in *Norsk. Mag. f. Lægevidensk*, 1874, and more fully in *Virchow's Archiv* for January, 1880. He found constantly in the nodules of leprosy, both rod-shaped and granular bacteria, enclosed in certain brown-colored cells.

Dr. Albert Neisser, then of Breslau, now of Leipzig, extended these observations of Hansen's in a study of leprosy pursued in Granada in the winter of 1880–81, employing, for the first time, the staining methods of Weigert and Koch. Neisser claims to have been the first to declare that there exists in leprosy a specific form of bacteria which is the direct *cause* of the disease, while Hansen had originally merely suggested that such relation might exist. Neisser also made a culture of the bacillus.

In May, 1881, Dr. I. Bermann, of Baltimore, succeeded in demonstrating the bacillus lepræ in the "lepra cells" in sections of skin removed from a leprous patient. Recently (Dec., 1882) Hansen has published another paper in *Virchow's Archiv*, confirming and extending his original results by culture experiments. The names of Klebs and Ecklund have also been associated with studies on *bacillus lepræ*.

The *bacillus lepræ* is a peculiar one, being very attenuated and further characterized by black dot-like nodosities, sometimes at one end, at others at both, while sometimes there are found two or three intermediate dots, in which event the bacillus is always longer. These granules have probably something to do with spore formation, and may entirely replace the bacilli, which then appear to break up into them.

As to their occurrence, the bacilli, at least in the tuberculous form of leprosy, are found wherever the leprous productions occur, be they recent or old. This is especially true in the nodules of the external integument. Hansen has never seen the involvement of the liver, spleen, testicle, lymphatic glands, and nerves in its early stage; but when he has found these organs diseased, the bacilli were present in them. Nor has he ever seen the latter in the blood, though Köbner claims to have found them there in his patient. Nor did Hansen find the bacilli in the anæsthetic form of the disease, although his opportunities for the study of these have been limited.

An important link in the argument is, however, wanting, in that none of the attempts at inoculating leprosy in the lower animals, either by the introduction of pieces of leprous tissue or by the injection of the culture fluid swarming with bacilli, have been successful. It is true, Neisser claims to have communicated leprosy to dogs by inoculation, but both Köbner and Hansen, who acknowledge to have themselves failed to inoculate monkeys, consider Neisser's results doubtful and unsatisfactory. Of course, until more uniform success results from inoculation experiments similar to those which have followed the inoculation of *bacillus malariæ*, the existence of a specific bacillus cannot be admitted. In the above, however, we present our readers with what may be considered the present state of the question.

CHILDREN'S FOOD.

WE remember once going to see a respectable mechanic's child who had just recovered from an attack of cholera morbus. We found the boy of three years sitting at the tea table wrestling, and successfully, too, with a dozen fried oysters, a good-sized bowl of pretty strong tea, and a mug of beer. Thanks to our subsequent, but, we also regret to add, our unrequited, skill, he escaped the dangers

of a relapse, to fall a victim a year later to similar parental indulgence. Such an incident, together with many another drawn from late Christmas experience, may well point a moral as to the food of children.

Up to two years of age, little besides milk should be given. Before this age the stomach cannot bear stronger food. Even after it, and up to adolescence, great care is required in the choice of the diet. It should be simple in *quality*. Milk, oysters, eggs, plain farinaceous foods, easily digested and simply cooked meats, these should constitute the staple. Even if the children eat at the family table—an American habit we most heartily commend, because of its happy influences on both parents and children—they should be restricted to the simpler foods. Indeed, it is to be hoped that the very difficulty of enforcing such restrictions may lead to the abandonment of the richer dishes rather than to the exclusion of the children.

But it is especially the villanous but delightful concoctions at dessert that we must condemn. Pies and doughnuts are bad enough, but the wonderful combinations in various "sweets," to tempt an already satisfied appetite, are well-nigh a dietetic Pandora's box to all—except the doctor. Like St. John's little book, they are sweet in the mouth, but bitter in the belly. Children should never touch them any more than the rich ragouts or the highly spiced dishes of the rest of the meal. Fruit, ripe and wholesome fruit, varied so happily in this country from month to month, should be the usual dessert, with occasionally simple puddings and the plainer cakes. The hot biscuits and various forms of breakfast cakes we would not austerely exclude the whole year round from the older children, but let them be enjoyed as rarities.

The *quantity* of food eaten is not nearly so important as its quality. Children in good health will not often overeat if the food be simple. It is the enticing superfluities that do the mischief. Fear not only "gift-bearing Greeks," but gift-bearing cooks as well. If the appetite flag, and too little food be eaten, a little beef tea, tid-bits, and varieties may be used, which need not be unwholesome because uncommon; for a time a little stimulant may do good.

Many children, like many horses, thrive well on but little food. Personal idiosyncrasies must be taken into account. The scales are the best test. So long as a child gains in weight, even only slightly, parents need not, as a rule, have any anxiety.

The *regularity* of children's diet is also of prime importance. The stomach needs its periods of rest as much as the brain or the muscles. Feeding between meals, even if the food be wholesome, is noxious; not but that occasionally some good bread and butter, or a little fruit may be proper for a growing, romping child, but the rule should be the other way. To give candy, doughnuts, cakes, pie, etc., between meals, is unintentional cruelty. It not only cheats the stomach of its needed period of rest, but destroys the appetite for the succeeding meal. Candy in moderation, and as a dessert, may be allowed as a venial sin—certainly a winsome one. But it should be well chosen, and bought only of reliable dealers. Not a little arsenic or other poison sometimes lurks there.

When we say "between meals" we do not mean that the meals shall only be three in the day. Growing children need at least one lunch, especially if the interval between breakfast and dinner be a long one. Very often children in private schools do not get dinner until two or three o'clock. Six or seven hours between meals is too long an interval without food, and every child so situated should be supplied with a hearty lunch at recess.

The general tenor of the above remarks applies equally to children's *drinks*. Milk or water may always be given. In winter, when something hot at breakfast is desirable, chocolate and its allies may be used with advantage. Tea and coffee should never be given to young children, and only in moderation, if at all, before twenty years of age, except occasionally in cases of sickness. Among the poor especially, as our opening story shows, the vicious habit of giving tea in large quantities to young children is common, and it cannot be too strongly condemned. Beer, wine, and all the stronger forms of stimulants are, *ipso facto*, the more to be condemned. Apart from the moral dangers they are harmful physically. The parent who gives them commits well-nigh a crime.

EXPERT UNEXPERTNESS.

IN a recent issue of one of the journals devoted to specialties in medicine, we find a case recorded under the title of "post-scarlatinal insanity," in which the author expresses himself as having experienced serious doubts whether the case should be regarded as post-epileptic or post-scarlatinal insanity. Our readers who are not neurological experts, but who do know something of general medicine, would not be surprised to encounter a case of convulsions in a girl of nine years, during the desquamative period of scarlatina, or that a joint affection had supervened in the same case. But by an expert these ordinary complications are narrated with an impressive gravity befitting an account of unprecedented phenomena. The forms assumed by the delirium are described with a pompous formality of sounding epithets. Thus, the girl of nine "manifested rudimentary delusions of

grandeur strongly tinged with imbecility." In another place, "there is evident amnesic aphasia with word deafness," because, forsooth, "in answer to questions she invariably responds 'flees.'"

We learn further that "to anticipate several questions which are sometimes asked, I will say that no sphygmographic, electrical, or craniometrical examinations were made." Our readers will surely regard our statement as incredible when we say that the account contains not one word of the state of the urine. Notwithstanding the fanfaronade of technical phrases, there is not a word in respect to the condition of uræmia, which was, doubtless, the true explanation of the convulsions and of the delirium. Surely it does not require an extraordinary expertness to diagnosticate the uræmic complications of scarlatina, but it would appear that the psychiatric expert can find no organ diseased except the one with which he is concerned, and that all others are of little importance in causing any form of symptomatic disturbance.

A GRIEVANCE OF PASSED ASSISTANT SURGEONS IN THE NAVY.

MANY passed assistant surgeons now in the navy have been recently deprived of their seniority, fairly assigned them after competitive examination in accordance with a custom prevalent since 1824, which is recognized in legislative Acts of 1828 and 1835, through a literal construction by the Attorney-General, on February 25, 1881, of a law of March 3, 1871, as epitomized and presented in the Revised Statutes of the United States in 1877.

In order that they may have restored to them the seniority in their class of which they have been thus unjustly, though legally, deprived, they have petitioned Congress to enact, "That Passed Assistant Surgeons in the Navy shall be arranged upon the Navy Register according to merit, as determined by the system of competitive examination which has heretofore prevailed; and that this Act shall apply to the Passed Assistant Surgeons now borne upon the Navy Register, and all future assignments to positions on said Register shall be made pursuant to such competitive examination."

The appointment and promotion of assistant surgeons in the navy in accordance with the results of competitive examinations have had, it is universally admitted, the effect of procuring for the naval service better qualified practitioners than it could have obtained otherwise. This competitive examination presents the strong motive of personal interest to induce the competitors to strive zealously to obtain seniority by increasing their professional acquirements, because the higher the seniority the earlier is the promotion to the grade of surgeon. The measure asked for is surely proper, and it is therefore hoped that it will be speedily granted.

THE GARFIELD BOARD OF AUDIT.

THE award of $20,500 to the four surgeons summoned to attend President Garfield has been received by them. In response to the question of a newspaper interviewer, Dr. Hamilton is reported to have said that he accepted his cheque in much the same spirit as he would a dividend from a bankrupt corporation.

We are informed that the report of the Board of Audit has been called for in the House. This will again bring up the whole subject, and, in reaching a conclusion, Congress will have the aid of the light thrown upon the value of professional services as shown by the sums recently paid, without demur, by the Government to the lawyers in the Guiteau and Star Route trials. Congress will then also have it in its power to do tardy justice to the surgeons who are compelled to accept whatever may be awarded them, since, generously confiding in the justice of the Nation, they, by request, signed away all claim upon the estate of President Garfield, as a prerequisite to the case being brought before Congress for settlement.

Members of Congress, who are for the most part lawyers, have shown, perhaps not unnaturally, a willingness to make liberal compensation for legal services rendered to the Government, and we hope that they will now embrace this last opportunity to make, at least, a just remuneration to the eminent surgeons whose professional services were unsparingly given to our late President, under particularly trying circumstances.

THE new contribution to cardiac therapeutics, *Convallaria maialis*, has been carefully studied by Dr. Desplats, and his results are given in a special memoir. He concludes that convallaria is practically useless in ascites due to cirrhosis, and is of doubtful utility in albuminuria, and in cardiac icterus (nutmeg liver).

In cases of obstruction or regurgitation at the mitral orifice, with visceral congestions, dropsy, etc., convallaria lessens the number and improves the strength of the cardiac contractions, increases the urinary discharge, and thus removes the congestions and dropsy. It is, therefore, distinctly useful in this group of diseases. In his treatise on the diagnosis and treatment of heart diseases, Prof. G. Sée reaffirms his original opinion in regard to the utility of convallaria, which was strongly expressed in favor of its use in the group of cases for which it is advised by Dr. Desplats. Sée regards it as superior to digitalis in cardiac dyspnœa, in which he combines it with the iodides.

SOCIETY PROCEEDINGS.

NEW YORK SURGICAL SOCIETY.

Stated Meeting, December 26, 1882.

THE PRESIDENT, T. M. MARKOE, M.D., IN THE CHAIR.

EXCISION OF THE ARTICULAR EXTREMITIES OF THE PHALANGES.

DR. POST presented a patient upon whom he had operated for the relief of an angular deformity, of about fifty degrees, of the index finger of the left hand. It was one of a class of cases in which he had operated a considerable number of times within the past few years, by excising the end of the phalanx. About fourteen years ago this patient had his finger dislocated while playing base-ball. The dislocation was not reduced, and the terminal phalanx remained flexed at about the angle mentioned. For some time before he came under Dr. Post's observation there had been suppurative disease, and on examination there were found two sinuses leading down to rough bone. The operation, therefore, was performed not merely for the correction of the deformity, but for the removal of the carious portion of bone, and in this instance instead of excising the distal end of the proximal bone, he excised the diseased portion of the ungual phalanx. The operation had been performed a little more than three weeks, during which time the finger had been supported by a splint. Three days ago the splint was removed, and the parts were supported by adhesive plaster. The wound healed readily, and the deformity had been corrected. Dr. Post said that the operation in his experience had not been attended by so much irritation as had amputation of the phalanx. He had applied the same operation to the phalanges of the toes. He had not met with a case in which there had been any considerable amount of irritation following the operation. He believed that the wounds healed quite as readily as after amputation. He had not succeeded, of course, in securing a perfect joint, although there had been in nearly all the cases some motion. The after-treatment consisted in keeping the parts supported until they became sufficiently firm to sustain themselves.

The PRESIDENT then read a paper on

TRIPIER'S MEDIO-TARSAL AMPUTATION.

During my residence at the New York Hospital, in the years 1839, '40, '41, one of our best surgical nurses, a man named George Compton, had lost both of his feet by amputation through the tarsus by Chopart's method. He wore on each foot a shoe, not very ingeniously contrived, nor very neatly constructed, but one which, nevertheless, by much forbearance and not a little suffering, he was able to wear occasionally, and to stump about his ward with considerable agility, which certainly was the reverse of graceful. I remember that he used to pad his shoes very carefully with layers of old blankets, cut to fit the stump, and then lace them very tightly around the ankle and part way up the leg. Thus provided, he was able to get about with some comfort for a certain time, but soon the pressure upon the cicatrices gave rise to pain and soreness, frequently terminating in most intractable ulceration, and it must be acknowledged that, brave as he appeared in his shoe toggery on Sunday mornings, as he walked down the front avenue leading to Broadway, he would commonly come back in the evening wearied and limping, and glad enough to get rid of his uncomfortable shoes, and only too happy to be allowed to stump about on his knees. Of course, he was not at all times equally disabled. Several of the surgeons of the hospital tried to improve his stumps by various operations and modes of treatment, but without any permanent benefit, and to the last his active and useful life was rendered miserable by the constant irritation of ulcerated and tender stumps, and his intelligence and courage and endurance only showed what a prize he might have won in life's race if he had not been so heavily handicapped. I did not then study the position and relation of the bones, as I should be only too happy to do now; but this I remember, that his chief trouble arose from the frequent and intractable ulcerations of the face of the stump, showing that the cicatrix bore the chief weight of the body when he walked, and was, therefore, constantly suffering from the effect of pressure directed immediately upon it.

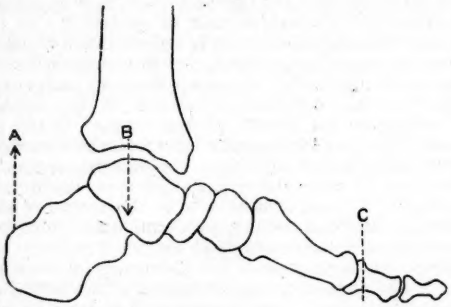
This unfavorable behavior of the stump left by Chopart's amputation has been noticed by all the surgeons who have had much to do with this operation, and much thought and ingenuity have been expended in endeavoring to explain and to obviate it. In regard to the cause, all seem to be agreed that it depends upon a change in the position of the stump whereby the heel being forcibly drawn up, the front part, or the cicatricial face, is thrown downwards, so that, instead of resting on the natural plantar cushion of the inferior surface of the stump, this anterior face receives the whole weight of the body. It is somewhat remarkable that so wide a diversity of opinion should exist among really good observers as to the reason of this change. It would seem to be a simple and adequate explanation of the deformity to say that it is due to the unopposed contraction of the gastrocnemial muscles, but I find that, at a recent meeting of the Société de Chirurgie in Paris, a M. Larger read a paper on the causes of the tilting of the heel after partial amputations of the foot, and arrives at the conclusion that it is due solely to the atrophy of the anterior muscles of the leg, and not at all to the action of the calf muscles, which, he asserts, take no active part in the change of the position of the heel. Acting upon this theory, he applies electricity to the weakened muscles, and insists that any other treatment is entirely unphilosophical, and therefore useless. The report of the committee to whom this paper of M. Larger was referred, did not accept the exclusive views of the writer, and while they acknowledged the possible occurrence of the atrophy in question, they felt that they could not ignore the powerful influence of the muscles of the calf. In the course of the discussion which followed, M. Verneuil contended that both the anterior and the posterior muscles of the leg undergo atrophy after these amputations, and that therefore neither of them have anything to do with the displacements in question.

By all the older, and by far the larger number of the best modern authorities, the calf muscles are fully recognized as the active agents in producing, or at least in perpetuating, the deformity. The main practical question, it seems to me, is what other vital or mechanical conditions favor, though they may not originate, the undesirable position which the fact is so apt to assume. That there are other causes which at least contribute to this undesirable result would seem to be proved by the fact that division of the tendon has been recommended by many of the best operators, and yet it is universally acknowledged that this procedure does not by any means always prevent the deformity, and still less cure it when it has occurred. Let us look now at one of these accessory causes, to my mind a very important one, and see what practical relations it may have to the main cause, and to the prevention of the trouble.

While the patient is in a standing posture, the tendency of strong contraction of the gastrocnemial muscles

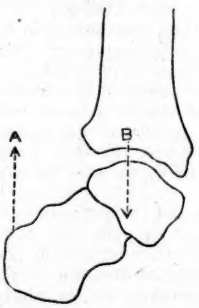
is to draw up the heel, and thereby extend the foot upon the leg. This movement is resisted by the long lever of the foot pressing upon the ground, and thereby preventing the extension from taking place. The only effect then of moderate action of the sural muscles is to keep the foot firmly planted on the ground, and at the same time to prevent the body from falling forwards. If now the action of the calf muscles be much increased, the ball of the foot remaining in contact with the ground, the effect will be to raise the posterior part of the foot, and with it the whole body, which rises as the act of extension of the foot on the leg is accomplished. The action results from the power A acting on the short arm of the lever moving the resistance B on the fulcrum C, which is at the end of the long arm of the lever (Fig. 1).

FIG. 1.



Now let us suppose, as happens in Chopart's amputation, that the long arm of the lever is removed by section between the scaphoid and astragalus; and then we should have the power A unopposed by the fulcrum C, and therefore unable to counteract the resistance B, which would force the astragalus downward until it came in contact with the ground. This becomes plain by glancing at Fig. 2.

FIG. 2.



If now the unopposed power A continues to act, its only further effect, after the astragalus has reached the ground, must be to raise higher the heel and thereby roll the astragalus in its mortise between the malleoli, so that its anterior face looks more and more downward, until finally it comes to press directly on the ground. It must not be forgotten, however, in thus estimating the action of the sural muscles, that if their influence should be entirely eliminated (as by tenotomy, for example) there would still remain in the obliquity of the under surface of the bones remaining after a Chopart's amputation a mechanical reason why the heel should rise and the astragalus descend as soon as the weight of the body is brought to bear, as in the act of standing. These two causes, then, it seems to me, conspire to produce the result we are considering, viz., 1st, the unopposed contraction of the gastrocnemial muscles; and, 2d, the oblique surface, in reference to the level of the ground, presented by the inferior surface of the bones which constitute the stump. When now we add to these statements the fact that the ligamentous tissues around the ankle-joint soon become so contracted and so rigid in their changed position that, even when the tendons are cut, they will obstinately maintain the displacement, we have related the chief, if not the only, causes which produce or perpetuate the deformity now under consideration. A review of these different causes shows also why it is that the deformity is not in every case relievable by section of the tendo Achillis; and particularly why that operation, performed after the lark heel has become established, does in many cases so little to improve the usefulness of the medio-tarsal stump.

The study of the causes of the deformity of the Chopart stump has led surgeons to seek some plan by which it might be avoided, and various suggestions have been offered in that direction. The one to which I wish to call the attention of the Society is one which was first brought forward by M. Tripier, of Lyons, and which has since gone by his name. It may be considered as a modification of Chopart's amputation in so far that the astragalus and calcaneum are left; but it differs from it in this, that a segment is sawed off from the lower part of the os calcis in such a way as to leave the lower or plantar surface of the bone showing a line parallel with the surface of the ground on which it bears in standing or walking. This section is made at the level of the sustentaculum tali in such a direction as to bring the line of the saw-cut at right angles to that of the tibia, leaving therefore a bone surface which, when the weight of the body is brought to bear on the stump, will have no tendency to change its relation parallel to the ground on which it rests. This is the principle of the operation, and its originator claims for it a great success. The cases in which the operation has been tested, thus far, are very few in number, and time enough has not yet elapsed to enable us to reach a final verdict as to its merits. In the *British Medical Journal*, of February 26, 1881, Dr. P. J. Hayes, of Kingston, Surgeon of the Mater Misericordiæ Hospital, reports a case in which he operated by this method, the man making a good recovery, and walking without a crutch in a month. He has since repeated the operation with a like satisfactory result. Mr. Wagstaffe, in the *London Medical Record*, discusses the merits of this operation and approves of it as likely to prevent the tilting heel. He contrasts the operation with the sub-astragalar, and gives preference to the method of Tripier. He does not say that he has ever done the operation.

Tripier's amputation is performed by an incision which commences on the outer border of the tendo Achillis, on a level with the outer malleolus, passes downward and forward, skirting the malleolus about one inch below it, passes then along the outer border of the foot, rather more toward the dorsal than the plantar surface, till it reaches the base of the metatarsal bone of the little toe. The line of incision here turns upward and inward, passing across the dorsum of the foot till it reaches the base of the metatarsal bone of the great toe, and then turning downward and forward into the sole is continued across the sole, making a convex flap at least an inch longer than the dorsal. When it reaches the outer border of the sole, it turns backward and upward, and joins the original incision at an oblique angle on the outer side of the foot. The flaps thus marked out are raised so far as may be necessary to disarticulate between the astraga-

lus and the scaphoid and between the os calcis and the cuboid, leaving behind the astragalus and os calcis, as in Chopart's amputation. Then the plantar flap is carefully dissected from the lateral and lower surface of the os calcis, so that it can be exposed as high as the sustentaculum tali, this part of the operation resembling the dissection round the heel in Syme's amputation at the ankle-joint. When the lower part of the calcaneum is thus fully exposed, it is to be sawn through just below the level of the sustentaculum, so as to make the line of saw-cut as near as may be at right angles to the line of the tibia when the patient is standing, and therefore parallel to the line of the ground on which it is to be placed. The section of the bone is recommended to be made from within outwards, thereby more surely avoiding any injury to the posterior tibial artery, which, from its important bearing on the life of the plantar flap, is to be most carefully guarded. Having made sure that the section of the bone has been made in the desired line, the sharp edges and corners of the bone are to be removed with cutting forceps, and the flaps brought into place and united by many sutures. Proper drainage-tubes being placed, and a supporting dressing applied, which shall hold the lower flap well in its place, the operation is completed.

I performed the operation in the following case: John Ohmer, a German, æt. 33, a waiter by occupation, was admitted to the New York Hospital April 18, 1882. His health had been good until about four months previous, when, without any known cause, his left ankle became swelled and painful. The trouble rather slowly increased, he continuing to walk about for nearly a month, when his ankle became so distressing to him that he took to his room and finally to his bed. The further course of the disease was one of steady increase, with increasing and finally severe and constant pain. On admission the tarsal region was recognized as the seat of the disease, the swelling, however, extending up to the ankle-joint. The swollen parts were very hard and tender to the touch, and the whole region was hot and painful, but without much redness of surface. After a time obscure, deep fluctuation could be discovered, and some crepitation could occasionally be elicited by moving the metatarsal bones on the tarsus. Deep incisions were made, letting out a great deal of pus. The part was poulticed, and everything was done that we could think of to improve his general nutritive condition, but without result. The abscesses did not heal; fistulæ remained; pain was not mitigated; in short, the carious disease was making constant progress.

On the 20th of May the actual cautery was applied at several points on the dorsum of the foot, but, beyond relieved pain, no benefit could be traced. The question of amputation soon decided itself, and inasmuch as we felt confident that disease was confined to the anterior row of the tarsal bones, it was regarded as a suitable case in which to make a trial of Tripier's method. The amputation was done on the 7th of June, precisely in accordance with the description given above. Particular care was taken to make the plantar flap as liberal as possible. All the fistulæ leading down to carious bone were included in the part removed, except one which remained on the inner side of the stump. When the disarticulation was accomplished the remaining bones were carefully examined, and were considered to be free from disease, and the section of the os calcis revealed bone tissue softened, indeed, from the atrophy of disuse, but as far as we could judge, free from present disease. The flap fitted exceedingly well, and covered the bone without the slightest tension. Everything proceeded to our entire satisfaction during the first few days after the amputation. The wound healed almost entirely by the first intention, and he improved very greatly in his general condition as soon as he was released from the irritation caused by the carious bones. The local conditions, however, did not long continue favorable. Part of the wound broke open and gave issue to pus, which, it was soon evident, came from a deeper source than the surface granulations. The stump became swollen and painful and it became clear that carious disease had attacked the astragalus, and probably the os calcis. His general condition began now pretty rapidly to depreciate, and though the operation wound was for the most part soundly healed, we were convinced that there was hopeless local disorganization of the bones, and that reamputation was the only thing that could save his life. The limb was accordingly amputated through the lower part of the leg on the 8th of August, just two months after the previous operation had been performed. He made a rather slow, but ultimately a perfect, recovery.

As far as the cure of the disease was concerned, Tripier's amputation, in this case, was not a success; the mischief spreading to the bones left in the stump, and requiring another operation for its eradication. This feature has, however, no particular bearing on the operation itself, which is the subject we are now considering, and it may be said generally that in all these partial operations on the foot and also on the hand, some uncertainty must exist—first, as to whether any disease of the bones has been left behind; or, secondly, whether the bones, now healthy, may not take on diseased action in the future. As a rule, I think this point ought, with care, to be determined with substantial accuracy, and, in point of fact, recurrence of disease has been found to be rare. I am aware that Mr. Syme says, "In this operation the astragalus and os calcis are left, and though they appear sound at the time of operation, yet the disease frequently recurs, as you may learn from the fact that I once performed three secondary operations within the period of twelve months upon stumps in which Chopart's operation had been practised." On the other hand, Mr. Hancock replies: "This objection, thus forcibly urged by Mr. Syme, is, I am aware, maintained by other surgeons, but it is not supported by the statistics of the operation; since, of 104 cases, exclusive of those mentioned by Mr. Syme, I find only two followed by secondary amputation, and this for retraction and painful cicatrix, and not for recurring disease."

The new operation seems to me to offer a satisfactory substitute for a very unsatisfactory one, such as in many cases Chopart's amputation is conceded to be. It is an operation not yet proved, scarcely fairly tried, and it would therefore be unwise to pronounce too positively on its merits. That it is feasible, that it makes a good stump, is settled by the few cases in which it has been done. That it certainly and perfectly obviates all the disadvantages of the amputation which it is proposed to displace, we cannot be sure. I can only say that, as far as theoretical considerations go, and as far as a very few cases can be trusted, it seems to promise well, and I challenge for it the careful consideration of those who may have occasion to perform a medio-tarsal amputation.

DR. POST referred to another operation which furnished better results, so far as usefulness of the limb was concerned, than did Chopart's—namely, that performed by Dr. Quimby, also claimed by Dr. Turnipseed, of South Carolina. It consisted in a modification of Pirogoff's operation, but differed from it in the fact that the lower extremities of the tibia and fibula were left entire, the cut surface of the os calcis being brought in contact with the cartilage covering the lower extremity

of the tibia. Dr. Quimby had performed the operation three times and Dr. Post had performed it twice, and in all the cases there had been a good stump and no inconvenience had resulted from want of homogeneity of the parts brought together. The wounds had healed kindly, the stumps had been firm, and the operation had been attended by only a moderate amount of shortening.

DR. L. A. STIMSON said the idea of the operation referred to by Dr. Post was not novel. It had been considered by Pirogoff himself, and, he believed, also by the English surgeon Ure, who devised the operation at about the same time as Pirogoff. The suggestion has been frequently referred to and generally condemned. The lower end of the bone is sawn off in Pirogoff's amputation, not because surgeons do not know that bone and cartilage can unite, but mainly because of the difficulty of otherwise keeping the end of the calcaneum from being drawn up by the tense tendo Achillis, the same reason that has led to its modifications in the direction of the line of section of the calcaneum or of both bones. Dr. Stimson asked if it had been found difficult to perform Tripier's operation.

The PRESIDENT remarked that there was no greater difficulty in the performance of the operation than obtained in Syme's or Chopart's, except with reference to following the line of the incision.

DR. MCBURNEY had performed Chopart's operation in one case, in which there was ankylosis of the ankle-joint, not produced by disease, but by long confinement in one position. The patient made an excellent recovery and had a useful stump. That fact suggested to him the question as to whether sufficiently strict attention had been paid to retaining the stump in proper position for a sufficient length of time after Chopart's operation had been performed. The idea was to retain it in position for a sufficient length of time to allow a certain loss of muscular power to take place, so that there would not be a marked antagonism between the anterior and posterior groups. He could easily imagine that the stump after Chopart's operation might bend downward within two weeks if allowed to follow its own course. He asked Dr. Markoe if he was able to give any information upon that point.

The PRESIDENT replied that, so far as he recollected, none of the authorities had directed attention to the question raised by Dr. McBurney. It seemed to him, however, to be a reasonable suggestion, and it also seemed to him that the mechanical factor, even with atrophy of both sets of muscles, the os calcis touching the ground first, was the great factor of the operation. Contraction of the muscles was probably another important factor.

DR. SANDS believed that the neglect to prevent this extreme extension of the foot after Chopart's operation had been recognized by European continental surgeons as perhaps the main cause of the subsequent difficulty in locomotion. He believed it had been claimed that, if extension of the foot was duly prevented during the course of the treatment immediately following the amputation, it would not subsequently take place to such a degree as to bring the cicatrix in contact with the ground, provided the former was not placed too near the plantar surface. He believed that the verdict given by American surgeons against Chopart's amputation had not been sustained by statistics. The statistics given by Max Schede proved that the limb, after Chopart's operation, was no more frequently useless than after other partial amputations of the foot; there were only ten per cent. of useless limbs after Chopart's operation. He thought that Chopart's operation was entirely legitimate, provided the astragalus and os calcis were both sound. There was an objection to both of these operations, Chopart's and Tripier's, namely, that when carious disease invaded the tarsal bones, it was apt to implicate, sooner or later, all of them, and thus there might occur, as in the case related, extension of the disease after performance of the operation, so as to render necessary amputation at a higher point. Some years ago, Mr. Hancock proposed, in cases of partial amputation, to regard the foot as a whole, and to operate without reference to the articulations, forming the flaps and sawing the bones as might be expedient. Dr. Sands thought that the suggestion made many years ago was particularly useful in these days of antiseptic surgery, when operation wounds, whether of bones or joints, were so likely to pursue a favorable course.

DR. WEIR supplemented Dr. Sands' remarks by the statement that Hancock also suggested the removal of a small section of bone, so as to counteract the contraction of the posterior group of muscles.

DR. BRIDDON said that within the last two weeks a patient came under his observation who had had Chopart's operation performed two years ago. Since that time he had been incapacitated in going about on account of painful ulceration in the line of the cicatrix; the whole of the anterior surface of the stump was covered with cicatricial tissue, and a spot about the size of a quarter of a dollar was in a state of ulceration. He thought the ulcerated condition was not attributable entirely to the operation, inasmuch as the operation had probably been performed in apparently healthy tissues. The original injury, however, was a railroad accident, and he, therefore, presumed that, as was commonly the case in railroad accidents, the vitality of the tissues was, to a greater or less extent, destroyed to a point higher than that at which the amputation was made, as some sloughing of the flaps took place, making them scanty and tense, and ulceration occurred after repeated cicatrization. As a further operation, he had recommended Syme's amputation.

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

Stated Meeting, January 3, 1883.

THE PRESIDENT, W. S. W. RUSCHENBERGER, M.D., IN THE CHAIR.

DR. JAMES C. WILSON made some

OBSERVATIONS ON THE MANAGEMENT OF ENTERIC FEVER ACCORDING TO A PLAN BASED UPON THE SO-CALLED SPECIFIC TREATMENT,

which he has employed during the past year, and which, tested by such uncertain but not necessarily fallacious means as are available for a limited series of cases, has yielded satisfactory results.

The Plan of Treatment.—The management by medicinal means, super-added to the so-called rational and expectant method in general use in this community, differs from the common practice, and constitutes the plan in question. So soon as the patient's symptoms warrant a reasonable suspicion that he is about to develop enteric fever, he is put to bed, ordered a diet consisting of milk, animal broths, jelly and simple custards, in small amounts and at intervals of two or three hours. At night he is given a dose of calomel. This dose varies in amount from 7½ to 10 grains, and is repeated every second evening until three or rarely four doses have been administered in the course of the first six or eight days. It is given alone or in connection with sodium bicarbonate. There is commonly a slight increase of diarrhœa, if it be present, without aggravation of the other symptoms, and in some instances the tendency of the temperature at this time to steadily rise appears to be controlled. If, as is frequently the case, spontaneous diarrhœa has not re-

curred in the first week, the calomel usually brings about two or three large evacuations on the day following its administration, not more. In either case, the tendency to frequent passages in the later stages of the attack is favorably influenced by the repeated administration of this drug during the first week. If the case does not come under observation until after the tenth day, one only, or at most two doses of calomel are given. No further doses of it are, however, given during the course of the attack unless constipation occur. In this event, if the evidences of extensive or deep implication of the intestinal wall, such as abdominal pain, tenderness, or marked tympany are absent, calomel in 7½-grain doses is given at intervals of three or four days. If there is reason to suspect serious intestinal lesions, the lower bowel may be more safely emptied of its contents every third or fourth day, by enemata of moderate size (8 to 10 fluidounces). It is necessary to bear in mind that the gravest lesions of the gut, leading even to hemorrhage and perforation, have occasionally been observed in cases characterized, not only by constipation, but also by an entire absence of pain or tenderness, and very moderate tympany. The danger of salivation from calomel in these doses in enteric fever appears to be slight. In only one case in sixteen were the mercurial fetor and slight swelling of the gums observed.

Excessive diarrhœa has been controlled by the use of opium, in suppositories or by the mouth, often associated with bismuth. It is an invariable rule that the patient be kept in the horizontal position and to the use of the bed-pan and urinal, from the time of the recognition of the disease until defervescence is completed. He is, however, turned upon his side from time to time, and made to maintain that position for twenty or thirty minutes, if necessary, being supported by the nurse.

From the beginning of the attack the following mixture is regularly administered in doses of one, two, or even three drops in a sherryglassful of ice-water after food, every two or three hours during the day and night.

℞.—Tinct. iodinii, ... f℥ij.
Acid. carbolici liq., ... f℥j.—M.

Unless some unusual circumstance occurs to render a change necessary, this medicine is not suspended until the attack draws to a close. It is well borne by the stomach, and in one case only has it been necessary to omit the carbolic acid on account of the disgust caused by its odor.

Partly for the sake of its favorable influence upon the skin and for the sake of cleanliness, partly because of its favorable though slight influence upon the temperature, the patient is to be sponged twice a day with equal parts of aromatic vinegar or alcohol, and cold water. If it is more grateful to him, this sponging may be done with tepid water.

When the evening axillary temperature reaches 104° F., quinine in massive doses, 24 to 30 grains, is given upon a falling temperature. I usually direct 8 to 10 grains to be given in solution at 5, at 5.30, and at 6 A.M. the following morning. Administered thus at the decline of the temperature in its diurnal revolution, these large doses of quinine depress it from 2.5° to 3.5° F. After the lapse of forty-eight to seventy-two hours, if necessary the dose may be repeated. If these doses be rejected by the stomach—an unusual circumstance—half the quantity of quinine may be administered hypodermically. For this purpose a citric acid solution is to be preferred. Since the adoption of the plan of treatment under consideration, I have not encountered cases attended with such hyperpyrexia as has rendered attempts to control it by cold baths necessary or even advisable.

The minor nervous symptoms are best held in check by skilful nursing. For the relief of the headache of the first ten days absolute quietude, a dim light, etc., are often sufficient; occasionally the bromides alone or in combination with chloral are required. Later in the course of the disease chloral is unsafe. From the end of the first week the patient cannot be left unattended even for a few minutes, without risk. Persons in whom delirium was only occasional and transient, have in many instances destroyed themselves during the momentary absence of the nurse.

Alcohol is not often indicated prior to the beginning of the third week. It is commonly administered, usually in small amounts, towards the close of sickness. Some patients do well without taking it at all. It is, of course, administered in accordance with well-understood indications upon the supervention of delirium, ataxic symptoms, and the evidences of failure of the forces of the circulation. The patients are carefully watched well into convalescence, and cautioned against too soon regarding themselves as restored to health.

The dangers of the establishment of a focus of contagion are guarded against by the systematic, thorough disinfection of the stools immediately after they are voided.

The considerations which led Dr. Wilson to adopt this plan of treatment are:

1. A feeling of dissatisfaction regarding the expectant method of treating enteric fever, based upon the high death rate.

The percentage of fatal cases rarely falls below 15 per cent., and often exceeds 25 per cent. Jaccoud, with a collection of 60,000 cases, observed a mortality of 20 per cent.; Murchison, in 27,051 cases, 17.45 per cent.; Liebermeister, in 1,718 cases at Basle, under an expectant plan, records 27.3 per cent. of deaths. But turning from broad generalizations to personal experience, who is there here that, many times elated by the happy issue of mild or average cases treated by the expectant plan, has not realized the sense of utter powerlessness attending it when he has stood face to face with cases in which *to do*, rather than *to wait*, has been necessary to save life?

2. Enteric fever is the very type of the general diseases, of affections *totius substantiæ*. The tissues are universally implicated in the morbid processes; no function of the body wholly escapes perturbation. For this reason plans of treatment suggested by the prominence of certain groups of symptoms, or by the known lesions of particular organs, even though of undoubted benefit as far as they go, are in theory unsatisfactory, because they are directed in effect against conspicuous manifestations of the cause of sickness rather than against the cause itself.

Whilst in actual practice the treatment by turpentine, by alcohol, by opium with lead, or the silver nitrate, or by agents capable of controlling the febrile movement, as quinine, digitalis, salicin, and the salicylates, even the cold-water treatment itself, although at times and in the hands of certain clinicians showing favorable results—all these have failed of general acceptance on the part of the profession.

3. The general character of the disease, the specific nature of its cause, the unsatisfactory results alike of an expectant and of a symptomatic plan of treatment, or rather of the two combined, have united to render the idea of a specific treatment, a true cure for enteric fever a most attractive one, to stimulate thoughtful observers to renew again and again the disappointing search for it. To this idea may be traced the treatment by the mineral acids, by chlorine water, by carbolic

acid, by quinine alone, by quinine and digitalis, by iodine, by the potassium iodide, by calomel.

4. Not only is the conception of a specific treatment for specific diseases a most attractive one, and the attainment of such a treatment for enteric fever brought within the bounds of a reasonable hope by the analogy of syphilis and the malarial diseases, but the search after it with due caution and judgment has also the warrant of the very highest medical authority.

The treatment advocated consists of the use of the two remedies that are proved to exert a favorable influence upon the disease, iodine and calomel, with the addition of carbolic acid in minute amounts. The results of this treatment encourage Dr. Wilson to hope that they will lead to its trial on a more extended scale. That it amounts to a specific treatment in the narrow sense is not affirmed.

The *total number of cases treated* by Dr. Wilson by this plan is sixteen; all recovered, one being now in the second week of convalescence. Of these, eight were severe, the temperature reaching or exceeding 104° F. Of these, one was characterized by uncontrollable vomiting in the third week. The patient retained no food taken by the mouth for five consecutive days. One case was very irregular in its course, and was complicated by an obscure abdominal abscess which discharged by the bowel. The temperature in this case on two occasions attained 105° F. A third case was prolonged by a severe relapse.

Of the eight cases in which the observed temperature did not at any time attain 104° F., one was complicated by crural phlebitis, and another by the occurrence of intestinal hemorrhage.

The average duration of the eight severe cases was about thirty-one days; that of the eight mild and medium cases was about twenty-five days.

Of the whole number, ten were treated in hospital, six in private practice.

In two cases the special plan of treatment was abandoned about the beginning of the third week on account of the supervention of unusual symptoms of great gravity. These related respectively to gastric irritability and an obscure abdominal abscess.

These sixteen cases are unfortunately not a consecutive series. During the year in which Dr. Wilson has had the opportunity of observing them, two other cases of enteric fever occurred in his hospital practice in which this plan of treatment was not employed. One was previously greatly reduced, and was not regarded as a suitable subject for a special treatment, the efficacy of which was not yet established. The other, with an obscure history of a sickness of many weeks, and a very irregular temperature, developed the typhoid eruption, and within forty-eight hours had general peritonitis. These two fatal cases have, however, no bearing upon the result of the treatment.

In private practice several cases of mild continued fever of long duration were treated upon this plan. They are believed to have been anomalous cases of enteric fever, but as the rose spots of that disease were absent, and their departure from the typical disease was wide, they were not included. They all recovered.

The result of this plan of treatment has not only been satisfactory in respect of the recovery of all the cases treated, an accidental circumstance not liable to mislead persons familiar with the disease, but it has also been satisfactory in respect of the general course of the attack, and the appearance of the patient. These were in the main, despite the severe type of the disease in several of the cases and despite the occurrence of grave complications, favorable. Dr. Wilson made this statement with due regard to the personal equation, and with no willingness to permit the observed fact to differ from the actual fact, for he desires any who may make trial of this plan to be more favorably impressed with the results of it, than with his account of it.

DR. ROBERTS BARTHOLOW said that this plan of treatment is in part the so-called "specific" method. The administration of calomel in full purgative doses during the first week serves a double purpose: it has an effect on the range of temperature, and it acts on the typhoid germs present and multiplying in the intestinal canal. The use of iodine—usually Lugol's solution—throughout the disease, is also one mode of the specific treatment. By the use of this medicine, it is attempted to prevent the multiplication of germs in the intestine, to check fermentation, and to maintain an antiseptic action in the blood. Although the existence of typhoid germs has not been proved, it must be regarded as possible. Klein, a few years ago, announced the discovery of the specific organism of typhoid in the affected intestinal glands, but Creighton, of Cambridge, showed that the supposed germs were produced by the mode of preparation. This *fiasco* threw great discredit on the whole question of germs. Nevertheless, the course of treatment directed against supposed germs—the antiseptic method—has had a most favorable influence on the progress and mortality from typhoid. Whilst the specific plan has been advocated in Germany, the Montpelier school has brought forward carbolic acid as the remedy, and the success which has attended its use has been really remarkable. Quite a different complexion has been put on the statistics of mortality since they began the use of carbolic acid. It is probable that the combination of carbolic acid and iodine gives better results than the use of either singly. According to my observation, this method of treatment diminishes the diarrhœa, lowers the fever, and renders the disease much less violent, consequently lessening the mortality. Dr. Wilson has, therefore, rendered us a real service by drawing attention anew to this plan of medication, and especially by supporting his position with valuable cases and statistics. Besides this use of medicines, Dr. Wilson's treatment contains many valuable suggestions and practical methods, which, no doubt, contribute materially to his success.

DR. J. M. DA COSTA spoke of the purgative treatment in enteric fever, as that which had been tried in the French hospitals, and for a time sanctioned by Louis. As regards calomel, it was partly by its purgative action that it was supposed to be beneficial. In his hands the calomel treatment had not yielded favorable results. He had found carbolic acid useful in controlling diarrhœa and in lowering the temperature. He had also employed thymol in one-half to one-grain doses. He suggested the use of this remedy in the place of carbolic acid, as more acceptable to the stomach.

DR. WILSON called attention to the fact that carbolic acid and like drugs probably exert a favorable influence upon the course of enteric fever by their power to stay the rapid decomposition of the intestinal contents, which, for lack of the antiseptic influence of the intestinal juices, the bile, etc., all of which are changed, is a secondary cause of irritation, diarrhœa, and tympany. Calomel also, he thought, probably exerted an indirect beneficial influence in the same direction.

CORRESPONDENCE.

THE APPROPRIATION FOR THE ARMY MEDICAL MUSEUM AND LIBRARY.

WE have received from Dr. A. B. Isham, of Walnut Hills, Cincinnati, the following note addressed to him

by the Honorable Mr. Butterworth, in relation to the note relative to the appropriation for the Army Medical Museum and Library, given in the number of this journal for December 30th, page 738.

We are very glad to learn that we have been misinformed as to the attitude of Mr. Butterworth relative to this interest. In the last number of this journal, January 13th, page 59, the full debate in the House relative to this appropriation was given, and Mr. Butterworth's remarks as there reported fully sustain the statements in his letter.

HOUSE OF REPRESENTATIVES U. S.,
WASHINGTON, D. C., January 6, 1883.

MY DEAR DOCTOR: Your letter of the 3d is received. THE MEDICAL NEWS was not possessed of accurate information. So far from reducing the appropriation, I moved its increase over what the Committee had allowed. I am aware of the great value of the Museum, and am only sorry that no suitable fireproof building has been provided for it.

I thank you for the kind expressions contained in your letter, and with best wishes, I am

Very truly yours,
BENJAMIN BUTTERWORTH.

A. B. ISHAM, M.D.,
(Walnut Hills) Cincinnati, Ohio.

MUSCLE-READING.

To the Editor of THE MEDICAL NEWS.

SIR: A recent issue of your journal contains a report of some of my more recent experiments in muscle-reading, which, together with the comments made thereon, are founded on a misunderstanding of the nature of my experiments.

I understand you to say that there is in these experiments danger of being deceived. This is precisely not the case. Muscle-reading experiments are as accurate as anything in mathematics; they are indeed a part of mathematics itself. The beauty and the power of these experiments in muscle-reading consist in their precision, in the impossibility of being deceived, in the certainty of the results, and in the firm knowledge which the experimenter has that he has not been cheated by anybody, not even by himself.

It is quite true that many of the phenomena of artificial trance or hypnotism can be simulated; but the phenomena of muscle-reading, whether done by a person in trance or out of trance, cannot be simulated, as all authors all over the world who have studied the subject agree; even a non-expert can understand this after a short process of experimentation. If those who have not an opportunity of experimenting themselves on this subject, or of witnessing the experiments of others, will read what I have published on the subject years ago in *The Popular Science Monthly* (February and July, 1867; and *Journal of Science*, July, 1881, London), and in other journals, or will read the reports of the members of the Royal Society of England (Francis Galton, Ray Lankester, M. Romanes, and Prof. Robertson), who experimented in the same direction (*Nature*, June 23 and July 14, 1881), and who obtained results precisely similar to mine so far as they went, they will find that there is nothing in science more accurate than the results obtained in these experiments. The special object of these later experiments was to determine the extreme minuteness of the localities that can be found by means of artificial appliances. The details will soon be published in a scientific journal.

I hope, however, that the readers of your journal will not content themselves with the experiments of others on this subject, but experiment for themselves independently; subjects can be obtained everywhere, and they will find results as hertofore stated. I trust also they will do what is possible to enlighten the public, as well as professional and scientific men, on this theme. The strongest friend of quackery of all kinds, and the strongest enemy of our profession, in this country especially, but more or less in all countries, is ignorance of the action of mind on body. The best single answer to the delusions that sustain quackery that I know of is found in these muscle-reading experiments; in the rapidity, the precision, the delicacy, the certainty to which they can be carried on they show more powerfully than anything else I know of in science the interdependence of body and mind, and suggest the true explanation of the success of nearly all the forms of charlatanism in all ages. Withal, these experiments, which when they were first made were an original contribution to science, have a direct and positive value for physicians who rightly understand them; the suggestions they give are of practical as well as scientific service to our profession, and especially to psychologists.

It is now eight years since I made my first experiments in this department of science. The interest in the subject has increased to such a degree that there is already quite a literature on the subject, particularly in England, and in the past year the German psychologists are beginning to look into the matter. There is no danger that physicians will study it too much; there is much danger of studying it too little, and giving too little heed to the important practical suggestions which it offers. GEORGE M. BEARD.

NEW YORK, January 10, 1883.

NEWS ITEMS.

NEW YORK.

(*From our Special Correspondent.*)

A NUMBER OF SUPPOSED CASES OF TYPHUS FEVER were discovered in a Hester Street tenement house by the Board of Health, and it was found that there had been no less than six deaths in a few weeks. The disease was typhoid, and not typhus fever. The locality is one of the worst in New York, and this outbreak should be a warning to the authorities.

A NEW SOCIETY OF MEDICAL JURISPRUDENCE has been established by the dissatisfied seceding members of the Medico-Legal Society; among them Drs. Hammond, Spitzka, Morton, and a number of Brooklyn physicians. Dr. Wight, the Professor of Surgery at the Long Island Hospital Medical College, read a paper upon "Illusions and Hallucinations as Affecting Testamentary Capacity," at the inaugural meeting.

THE NEW CODE.—There is much sharpening of knives and burnishing of armor upon the part of the delegates who are to go to the State Society. The outside County Societies are repudiating the action of their delegates who went to Albany last year. Several medical men, not entirely unknown in medical politics and post-graduate instruction, are determined to make a bitter fight for the New Code.

THE ILLNESS OF MISS LILLIAN RUSSELL, a popular comic opera singer, brought forth a column of interview in the *New York World* the other day. Miss Russell seems to have been treated in sections by a dozen or more of the regular physicians and homœopathists, all of whom pursued a different plan of treatment, and abused each other roundly. The stage manager of Miss Russell objected because one of these gentlemen gave her 120 grains of quinine *per diem*.

THE GOVERNMENT'S MEDICAL EXPERTS IN THE GUITEAU CASE, many of whom gave a month of their time to the trial, have not been compensated yet, although over a year has elapsed. The lawyers have been paid, and it is difficult to see why an exception should have been made entirely in their favor.

POUGHKEEPSIE, N. Y.

(From our Special Correspondent.)

THE DUTCHESS COUNTY (N. Y.) MEDICAL SOCIETY AND THE NEW YORK CODE.—At the annual meeting of the Dutchess County Medical Society, held at Poughkeepsie on January 10th, it was

Resolved, That the Medical Society of Dutchess County instruct its delegates to vote in favor of repealing the Code passed at the last meeting of the State Society.

TROY, N. Y.

(From our Special Correspondent.)

RENSSELAER COUNTY (N. Y) MEDICAL SOCIETY AND THE NEW YORK CODE.—At the annual meeting of the Rensselaer County Medical Society, held January 9th, the following resolutions were adopted by the Society:

Resolved, first, That this Society affirms its loyalty to the Code of the American Medical Association.

Second, That it directs its delegates to vote for the restoration of the Code of the American Medical Association.

CHICAGO.

(From our Special Correspondent.)

PRELIMINARY EXAMINATIONS.—It has been known for a number of months that after the present course of lectures our State Board of Health would not recognize the diplomas of colleges which do not require an entrance examination of its students. Hereafter, graduates from colleges which do not have this requirement will be obliged to undergo a special examination before the State Board in order to be admitted to practice in Illinois. The four regular schools here require preliminary examinations.

TRAINING SCHOOL FOR NURSES.—This organization, now only about two years old, has become so much of a success at the County Hospital, that citizens have contributed funds enough to buy a lot and commence the building of a permanent Home for the school, within half a block of the college. Probably no good organization, touching in its work the practice of our profession, has ever been started among us which has worked more quietly and modestly than this, and certainly no other has, in so short a time, established itself on so firm a foundation of public confidence in its value and usefulness.

A NEW CHAIR OF LARYNGOLOGY.—Rush Medical College has just created a special chair of laryngology and elected Dr. E. Fletcher Ingals to fill it, with the title of Professor of Laryngology.

NEW ORLEANS.

(From our Special Correspondent.)

HEALTH OF NEW ORLEANS.—This city is not as healthy as it usually is at this time of the year. One hundred and sixty-three deaths occurred last week.

The most serious diseases prevailing are pneumonia, some of the eruptive fevers, and cerebro-spinal meningitis. It is reasonable to suppose that the holidays are in part chargeable with this increase of mortality. The festivities of Christmas and New Years bring such fruits as pneumonia and cerebro-spinal meningitis from exposure to cold; and the eruptive fevers by disseminating their special poisons throughout those assemblages which hilarious occasions bring together.

The deaths from smallpox average about ten a week; scarlatina is much less prevalent. Four cases of cerebro-spinal meningitis have been admitted to the hospital within the past ten days. One of these, which was apparently the most furious at date of admission, suddenly abated, and the patient has been discharged, well. It is a well-known fact that in some autopsies of patients who die from cerebro-spinal meningitis, no lesions are found to indicate that the inflammatory process had gone beyond the stage of engorgement. We may from this understand how it is that well-marked and threatening symptoms may sometimes stop short of death, or of such an amount of effusion as to make recovery slow. Two of these cases were under your correspondent's charge. They were treated by bromide, potash, and ergot in the first stage: by iodide-potash in stage of effusion.

Pneumonia is a fatal disease in all large hospitals. It becomes still more so if the hospital be located in a place where malaria is so rife that every case treated is complicated by its presence or influence. But in spite of these difficulties which we have to meet, the mortuary reports of the hospital show very favorably when compared with the death-rate of the city.

DEATH OF DR. WARREN STONE.—The early days of the present year instanced the death of Dr. Warren Stone, the son of the late Prof. Warren Stone. He was very largely endowed with the talents and noble traits of character which distinguished his father. The medical profession of New Orleans and a large circle of non-professional friends deplore his loss.

TORONTO.

(From our Special Correspondent.)

MORAL INSANITY.—At the December meeting of the Toronto Medical Society, the veteran Dr. Workman, late Superintendent of the Toronto Asylum for the Insane, read a long and very interesting paper on moral insanity, in which he said: "The term *moral insanity* was introduced into alienistic literature by Dr. Pritchard, an English writer, in 1835. Previously, Pinel, of Paris, to designate cases apparently identical, had styled them '*manie sans delire*.' His pupil, Esquirol, preferred the term '*manie raisonnante*.' This, to the vast majority of lay readers, must have appeared a very absurd designation, for the vulgar conception of insanity is that its subjects are incapable of reasoning, a belief which has been found very erroneous by many visitors of asylums who have ventured to address the inmates as if regarding them all as utterly mindless beings. Pinel's *manie sans delire* was a term quite applicable to a large proportion of all the insane, that is, in certain phases or intervals of the malady, but it would certainly be found to fail to cover the whole history of his cases. Insanity almost invariably has its inception in a change of the feelings or moral sentiments of its destined victims. This is its usual prodrome; but it is only when abnormality of the intellect has become manifest that the real disease is recognized by the friends of the patient, and too often, even then, it is not admitted by the neighbors, or the community, for outside of his own domestic circle the patient's language or actions may appear quite rational. When the insane commit crime, in almost all instances, the quality of the offence is judged of, not according to their actual mental state, but according to its nature or enormity; and it is very well known that the crimes of the insane are sometimes shockingly enormous. Why should they not be so?

"Pritchard's *moral insanity* might, with more propriety, have been called *insane morality*. Of the seventeen cases adduced by him, hardly one will bear the test of strict analysis. Some degree of intellectual debility or impairment may be detected, by an experienced reader, in every one of them, and several exhibited very palpable defects; one, indeed, perhaps his most accentuated, terminated in general paralysis, an invariably fatal form of insanity and one which, in its early stage, is often characterized by gross departures from moral propriety."

Dr. Workman, in the latter part of his address, gave the history of several cases of the *so-called* moral insanity which had come under his own observation, or had been recorded by his alienistic *confrères*. The details were very instructive. He concluded by earnestly admonishing his professional brethren, when unfortunately they might have to appear in Court as expert witnesses, never to fall into the inadvertence of using the term *moral insanity*.

INTERNATIONAL MEDICAL CONGRESS, EIGHTH SESSION; COPENHAGEN, 1884.—Prof. P. L. Panum, President of the Organizing Committee of the Congress, and D. C. Lange, Secretary-General, announce that, in accordance with the desire expressed by the International Medical Congress at its seventh session in London, 1881, that the eighth session of the International Medical Congress will be held in Copenhagen during the days from the 10th to the 16th of August, 1884.

FEES OF THE GUITEAU TRIAL EXPERTS.—The Department of Justice has decided on paying the experts summoned by the Government in the Guiteau trial at the uniform rate of $25 a day, with the ordinary witness fees in addition, but no other allowance for expenses. It was felt that the Government could not undertake to distinguish between experts according to their supposed rank in the profession or their comparative eminence, but must be governed by the length of their service. The bills ranged all the way up to $100 a day. The whole amount to be paid to the experts, exclusive of the ordinary witness fees, will be between $12,000 and $13,000.

Mr. Brewster Cameron, who has the matter specially in charge, intimates that any experts who were especially connected with the preparation of the case may receive an additional allowance on that ground. There were four of these—Dr. Gray, of Utica; Drs. Macdonald and Allan McLane Hamilton, of New York, and Dr. Kempster, of Wisconsin.

A TRAINING SCHOOL FOR NURSES is to be attached to the Kings County Hospital, on the plan of the one at Bellevue.

NATIONAL ASSOCIATION FOR THE PROTECTION OF THE INSANE AND THE PREVENTION OF INSANITY.—The annual meeting of the National Association for the Protection of the Insane and the Prevention of Insanity will be held at the hall of the College of Physicians of Philadelphia, on January 25, 1883, at 3 and 8 P.M. The address of welcome will be made by Dr. S. D. Gross. Papers will be read by Drs. Traill Green, J. S. Jewell, Charles K. Mills, Milner Fothergill, and others.

THE MONTHLY BULLETIN OF THE MORTALITY AND SANITARY CONDITION OF LOWELL, MASS., differs from most of its contemporaries, which are strictly statistical, in containing short but excellent articles intended for popular education in sanitary subjects. The issue for December discusses "Preventable Disease" and "School Hygiene."

THE JOHNS HOPKINS UNIVERSITY.—PRESIDENT GILMAN has just issued his seventh annual report, covering the session of 1881–82. The University is stated to be in a flourishing condition. There has been considerable original work accomplished during the year in the different departments.

DR. REMSEN'S attention seems to have been confined principally to analyzing the drinking waters of Baltimore and Boston with a view of remedying their disagreeable taste and odor. His work has been very successful, particularly at Boston, where he showed the cause of the trouble to be a fresh water sponge, *spongilla lacustris*. In addition to the general course in biology, there is a course preliminary to the study of medicine which has been well attended. It is hoped that the time is not far distant when the medical department in connection with the Johns Hopkins Hospital will be organized. The work accomplished here in biology has been extensive. Dr. Martin, since his connection with the University, has paid special attention to researches in physiology, and has devised a new method of studying the mammalian heart. He has succeeded in keeping the heart alive for five or six hours after the death of the animal, and now any external influences upon this organ can be investigated much more satisfactorily than heretofore, when biologists had to confine their experiments to the hearts of batrachia.

The appendix to the report contains a full list of Fellows since 1876, and a complete bibliography of all articles and books published by members of the University.

When it was determined to build the two new laboratories, the board of trustees was divided on the question of location, some favoring Clifton, others the present situation in Howard Street. As they are being built in the city, it would seem that the University will be permanently located in Baltimore, and not at Clifton, as was the founder's intention.

The Johns Hopkins Hospital is about a mile directly east from the University. The towers of the administration buildings can be seen from all prominent parts of the city. The buildings most needed at present are virtually completed. Seven of the twelve wards are almost ready. Five of these are free, and can accommodate with the greatest ease 156 patients. The other two are pay-wards and furnish room for thirty-two beds. These wards are to be supplied with all the modern improvements. The construction of the elaborate system of ventilation has been directed by Dr. John S. Billings, United States Army. Fifteen buildings have up to this time been erected, the ground inclosing them being a plot of fourteen acres. In addition to the wards the administration, nurses' kitchen, autopsy and medical school buildings are completed. All are of the Queen Anne style.

WEST VIRGINIA STATE BOARD OF HEALTH.—Governor Jackson in his biennial message to the Legislature, dated January 10, 1883, refers to the Health Board of the State as follows:

"The law establishing the State Board of Health and regulating the practice of medicine and surgery, as amended and re-enacted last winter, has proved a wise act of legislation. It is admirably adapted to secure the protection of the lives, health, prosperity, and happiness of all classes of the people. It has met with the sanction of eminent sanitarians in other States, who have become acquainted with its provisions. The law is now in force in every county of the State, and we may reasonably expect that its operations will prove of much benefit. The number of registered physicians and surgeons to date of report

is 1,041. Of these, 958 are residents of the State and the residue residents of adjoining States.

Besides the careful protection of the interests of the life and health of the inhabitants of the State, the Board is charged with the duty of investigating the causes of diseases occurring among the domestic animals. The value of this provision of the law was witnessed in Brooke County last summer, when what is known as the Southern cattle fever made its appearance in that county. The prompt action of the State and County Boards arrested the spread of the disease, and probably saved large sums of money to farmers of that locality, in the preservation of their stock from infection."

DISINFECTION BY HEAT, AND ITS PRACTICAL APPLICATION.—The superiority of heat over all fumigations and other reputed disinfectants is now generally recognized, and local sanitary authorities have already in many districts set up disinfecting ovens or chambers of one or other kind. It is, however, of the utmost practical importance that the intensity and duration of the temperature required for the destruction of bacilli and of spores should be fully appreciated and attended to, if such "disinfection" is to be really effective, and not a dangerous delusion. DRS. R. KOCH and WOLFFHUGEL, after a long course of carefully conducted experiments, find that bacteria are not killed until after an exposure of one hour and a half to a temperature of 100° C. (212° F.). Spores of fungi require a greater heat, viz., 110° to 150° C. (230° to 300° F.), for at least as long; while nothing less than a temperature of 140° C. (285° F.), continued for three hours, suffices to kill the spores of bacilli, as those of anthrax. These are the germs with which we have to deal in practical disinfection. Such a temperature will, in three hours, infallibly kill these spores, but since it takes that time to penetrate to the centre of even small bundles of clothes, or of pillows, a further exposure of like duration will be required for their complete disinfection, and few articles of clothing or bedding will bear such treatment without serious damage. It is to be regretted that Drs. Koch and Wolffhugel did not repeat their experiments with moist air, for there seems to be no reason to fear that the presence of a proportion of vapor not amounting to saturation can detract from, if indeed it do not actually enhance, the efficacy of the heat; while it is known that the most delicate fabrics, as ostrich feathers, dyed silks, etc., are uninjured by a temperature of 300° Fahr. under these conditions, and if the moist air be forced in under pressure, as in W. Lyon's apparatus, the penetration of the heat is greatly facilitated. The general conclusion to be drawn from these experiments, which are described in detail in the *Mittheilungen des k. Gesundheitsamte*, Bd. i., seems to be that nothing less than a three hours' exposure to a temperature little short of 300° Fahr., with precautions to insure the penetration of every part of the articles subjected to it, is sufficient. How this is to be attained is a question of detail, but at present it certainly appears that preference is to be given to the particular apparatus above named.—*Medical Times and Gazette*, Nov. 4, 1882.

SMALLPOX IN SOUTHERN AFRICA.—Reports recently received from Cape Town state that the epidemic with which the city and neighboring villages have been scourged during the past few months is now abating. There were admitted into the Lazaretto from the commencement of the outbreak until October 30, 1,015 patients, but these constitute only about one-eighth of the whole number who have been affected. Of the 1,015, 253 were white, and 762 colored. Of the whites, 183 had been vaccinated at some previous time, and 70 were unvaccinated. Of the colored, the corresponding numbers were 180 and 582. Among the whites 49 deaths took place, 23 of the patients in these fatal cases having been vaccinated, and 26 unvaccinated. Among the colored people 252 fatal cases occurred, 31 having undergone previous vaccination, and 221 never having been vaccinated.

	Admitted.	Died.	Percentage.
Vaccinated,	363	54	14.87
Unvaccinated,	652	247	37.88
	1,015	301	29.65

Difficulty was experienced in procuring reliable vaccine virus.

At St. Paul de Loundu the disease is progressing. During the month of October 150 cases of smallpox were registered, of which 80 proved fatal. The population is estimated at 12,000. The sanitary condition of the town is represented as of the worst possible character.

CHOLERA, PREVENTION AND CURE IN CHINA.—The United States Consul at Foochow, China, in a recent official report, gives information derived from native physicians concerning the cause and treatment of cholera. The disease is conceived to arise from unseasonable weather and a lack of harmony between the upper and nether worlds, so that men living between the two are exposed to and suffer from noxious influences; or it is caused by wraiths badly laid or of evil intent, who are boldly exorcised from houses and passages by nightly processions bearing the images of proper spirits. The villages quarantine against each other. Among the precautions to be observed in times of danger, we find that nothing must be eaten or taken into the stomach which is liable to cause diarrhœa or vomiting. Fats and oils must be avoided, and salted provisions used instead. Tea should be replaced by ginseng infusion. Exercise in the heated part of the day should be avoided. If all precautions fail, and the individual become affected, ginger, cassia, and aconite are administered, the black blood is let out from under the finger-nails by needle punctures, counter-irritation is applied to the arms by heated seeds, and finally, the umbilicus is burned to warm up the stomach and start the circulation of the blood.

HEALTH IN MICHIGAN.—Reports to the State Board of Health for the week ending January 6, 1883, indicate that consumption, erysipelas, measles, pneumonia, and bronchitis have increased, that diphtheria and neuralgia have considerably decreased, and that intermittent fever, whooping-cough, inflammation of brain, remittent fever, and influenza have decreased in area of prevalence.

Including reports by regular observers and by others, diphtheria was reported present during the week ending January 6th, and since, at 18 places, scarlet fever at 15 places, and measles at 12 places. Smallpox was reported in Richmond, Osceola County, January 2d, and in Royalton, Berrien County, January 4th.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JANUARY 8 TO JANUARY 15, 1883.

MUNDAY, BENJAMIN, *First Lieutenant and Assistant Surgeon.*—Assigned to duty at Fort Klamath, Oregon.—*Par. 3, S. O. 195, Department of the Columbia, December* 29, 1882.

SKINNER, JOHN O., *Captain and Assistant Surgeon.*—Granted leave of absence for one month.—*Par. 2, S. O. 5, A. G. O., January* 6, 1883.

JOHNSON, R. W., *First Lieutenant and Assistant Surgeon.*—Granted leave of absence for one month.—*Par. 2, S. O. 4, Department of Dakota, January* 5, 1883.

WOOD, MARSHALL W., *Captain and Assistant Surgeon.*—Granted leave of absence for one month.—*Par. 3, S. O. 4, Department of the East, January* 8, 1883.